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## EXPERIMENTS ON THE EFFICIENCY OF INCANDESCENT ELECTRIC LAMPS.

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II.

METHOD OF COMPARING CURRENTS.

There were thus two simultaneous measurements of current to be made. As there was, however, but one reliable ammeter at the writer's disposal, an arrangement was devised by means of which the strength of two electric currents—for example, the current through the lamp and the current through a resistance coil in derivation with the lamp—could be compared with each other by means of the Wheatstone's bridge.

As this method is the most accurate one known to the writer for comparing strong currents of electricity, and as it requires no special apparatus except an ordinary Wheatstone's bridge, it will be described in full, in the hope that other experimenters may find it useful. The arrangement will be understood from the accompanying diagram. Fig. 11.

The current C from the dynamo passes through the ammeter, and divides at the point *a* between the two branches *abe* and *ade* of the circuit, one portion of the current whose strength is denoted by $C_1$, passing through the resistance coil R, and the other, $C_2$, passing through the lamp, which stands in its place at the end of the photometer bar. *be* and *de* are two coils of heavy copper wire of small resistance, but exactly equal to each other; the resistance of each being denoted by $R_2$. After traversing these coils, the currents $C_1$ and $C_2$ unite at *e*, and return to the dynamo. *dc* and *ce* are two arms of the Wheatstone's bridge, whose resistances are marked $R_3$ and $R_4$. The bridge proper, containing the galvanometer, is from *b* to *c*, the circuit through it and the resistance coils $R_3$ and $R_4$ being closed in the ordinary manner by a double key, at *c*. When the key is closed, a small portion of the current $C_2$ is shunted around *de* through the coils $R_3$ and $R_4$ of the bridge. This current is denoted by $C_3$. Then the current passing through the portion *de* of the circuit is $C_2 - C_3$, as indicated in the diagram.

In using the apparatus, the arm $R_4$ is made a certain fixed resistance, say 1,000 ohms. The resistance $R_3$ is then adjusted, until, on pressing down the key, no current passes through the galvanometer. The points *b* and *c* are then at the same potential. It follows that the difference of potential from *b* to *e* is the same as the difference of potential from *c* to *e*. But the difference of potential, or the electromotive force, between two points of a circuit is the product of the current and the resistance between those points.

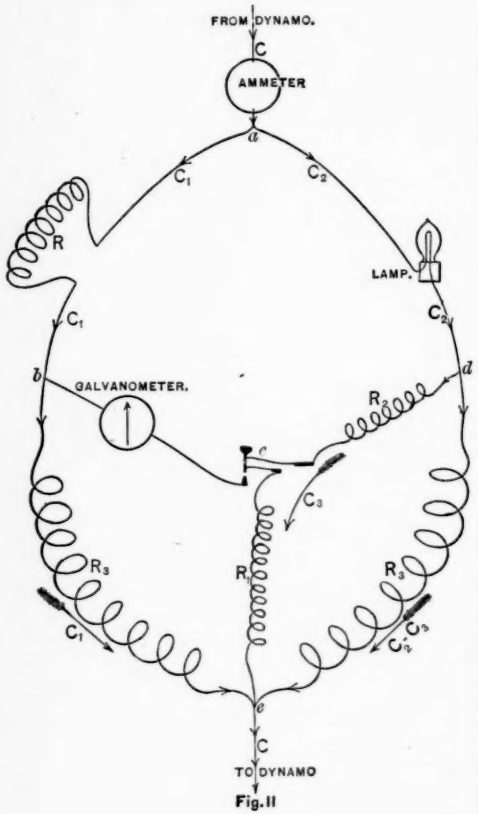


Fig. II

Therefore $C_1R_3=C_2R_1$, or $C_2=\frac{C_1R_3}{R_1}$.

Our purpose, it should be remembered, is to ascertain for each lamp the variation in candle-power and current strength as the electromotive force is increased; for which object it is necessary to vary the candle-power by a series of steps, and take simultaneous observations of the current and electromotive force at every point. Knowing, in each case, the value of the total current, C, if we can find the ratio between the currents $C_1$ and $C_2$ traversing the two main branches of the circuit, we shall have, not only the current flowing through the lamp, $C_2$, but—by multiplying the resistance of the branch *abe* by $C_1$—we may obtain the electromotive force.

Our object is now to find the ratio between the currents $C_1$ and $C_2$. It is evident that the electromotive force from *d* to *e* is equal to the sum of the electromotive forces on *dc* and *ce*; or using instead the products of the currents and resistances on these branches, we have

$$(C_2-C_3)\,R_3=C_3\,(R_2+R_1)$$

Substituting for $C_3$ its value $\frac{C_1R_3}{R_1}$ previously found, we have

$$\left(C_2-\frac{C_1R_3}{R_1}\right)R_3=\frac{C_1R_3}{R_1}\left(R_2+R_1\right)$$

Clearing of fractions,

$$C_2R_1-C_1R_3=C_1R_2+C_1R_1$$

Transposing and combining,

$$C_2R_1=C_1\,(R_1+R_2+R_3)$$

or

$$\frac{C_2}{C_1}=\frac{R_1+R_2+R_3}{R_1}$$

When the resistance $R_3$ of each of the equal coils of large copper wire is so small in comparison with the other resistances that it may be neglected, we may write

$$\frac{C_2}{C_1}=\frac{R_1+R_2}{R_1}.$$

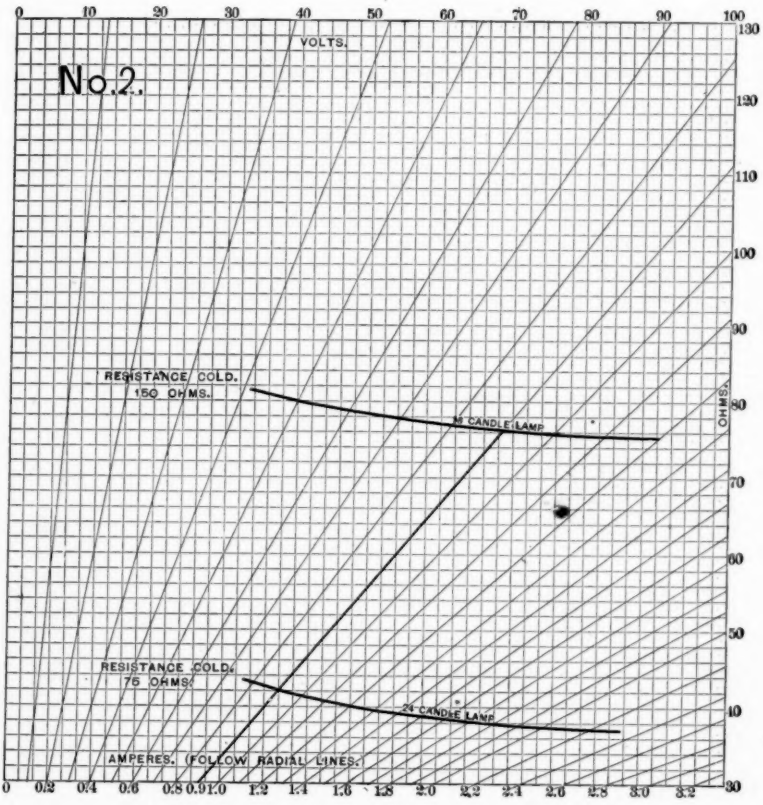
Expressing this equation in words, we say that the current passing through the lamp is to the current passing through the resistance coil R, as the sum of the resistances of the fixed and variable arms of the bridge is to the resistance of the fixed arm.

Having thus found the ratio between the currents in the two branches of the circuit, and knowing also by observation of the ammeter, the value of the total current C, which is the sum of the two portions $C_1$ and $C_2$, it is an easy matter to find the absolute value of the current in each branch of the circuit. The electromotive force on the branch *abe* is then computed by multiplying the resistance of that branch by the current passing through it, and this gives us the electromotive force between the terminals of the lamp, after deducting the trifling amount necessary to overcome the resistance of the wire in the branch *ade*. The horse power expended in the lamp is then found by multiplying together the

values of the electromotive force and the current, and dividing the product by 746.

The resistance $R_3$ of each of the equal coils *be* and *de* was in this case only 0.424 ohm, and the fixed arm of the bridge was generally made 1000 ohms. The value of $R_3$ was not neglected in the calculations, but as the error that would have been caused by its neglect would be in the fifth decimal place, its use was an unnecessary refinement.

through the coils of the bridge, we may, by adjusting the resistance of the fixed arm, reduce the current to a strength just sufficient to obtain deflections of the galvanometer; third, while the current passes constantly through the lamp, the circuit through the bridge is closed only when the key is depressed. It was found that, with ordinary care, the ratio of two currents could be thus ascertained accurately to four decimal places.




The above method of ascertaining the electromotive force and current of an incandescent lamp possesses several practical advantages over any method in which the lamp itself forms one arm of a Wheatstone's bridge. First, no special apparatus is required; second, instead of passing the entire current constantly

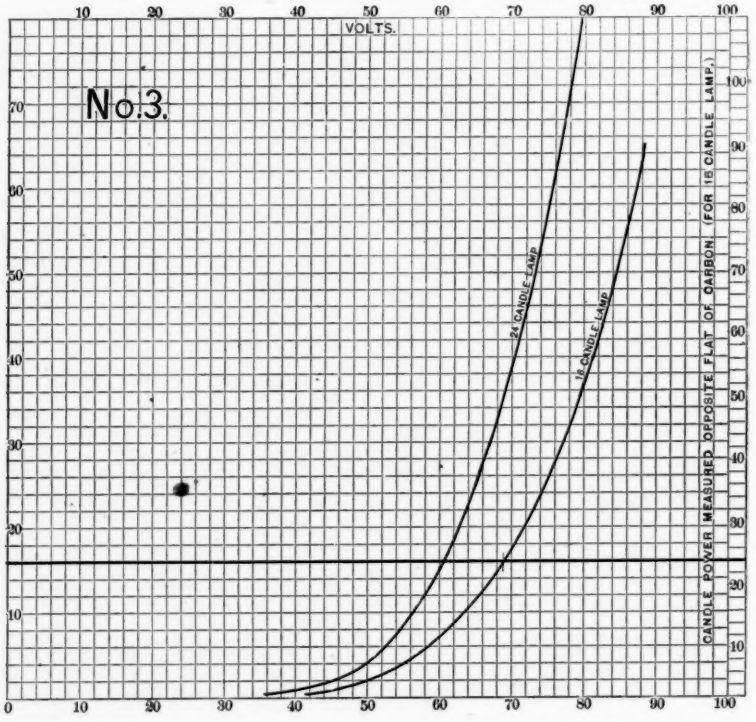
PRECAUTIONS OBSERVED.

The accuracy of this method of comparing currents evidently depends upon the exact equality of the resistances of the two coils *be* and *de*. They consisted of number six copper wire, covered with paraffined cotton braid. Such wire will carry fifty amperes without warming per-

ceptibly, and as the currents used in these experiments rarely exceeded two amperes, there was no danger from unequal heating of the two coils. Before beginning the tests, these coils were adjusted to equality by means of the Wheatstone's bridge, the final adjustment being made by a sliding contact resting on the wire, with the galvanometer so sensitive that a variation of $\frac{1}{100}$ of one per cent. was perceptible. Their equality was again tested at the close of the work. To insure that the two coils should always be at the same temperature, they were wound on a frame into a single loose coil.

It was also important that the resistance of the coil R should not be altered by the passage of the current. This coil consisted of about 200 ohms of number eighteen German silver wire. It was wound with silk, and hung loosely over a frame, so that the air could circulate freely through it. The current traversing it never exceeded one ampere, and was generally much less, so that it did not warm perceptibly. This coil was also arranged so that it could be instantly taken out of the circuit and connected with the bridge, and its resistance measured at intervals during the progress of a test. It was found that the error due to the heating of this coil never exceeded $\frac{1}{10}$ of one per cent.

This part of the work was carried on at




times by two observers, one of whom observed the ammeter and the bridge, while the other took simultaneous observations with the photometer. At other times the writer alone took all the observations, reading first the ammeter and the bridge, then the photometer, and then, to insure that no change had occurred, again reading the ammeter and bridge.

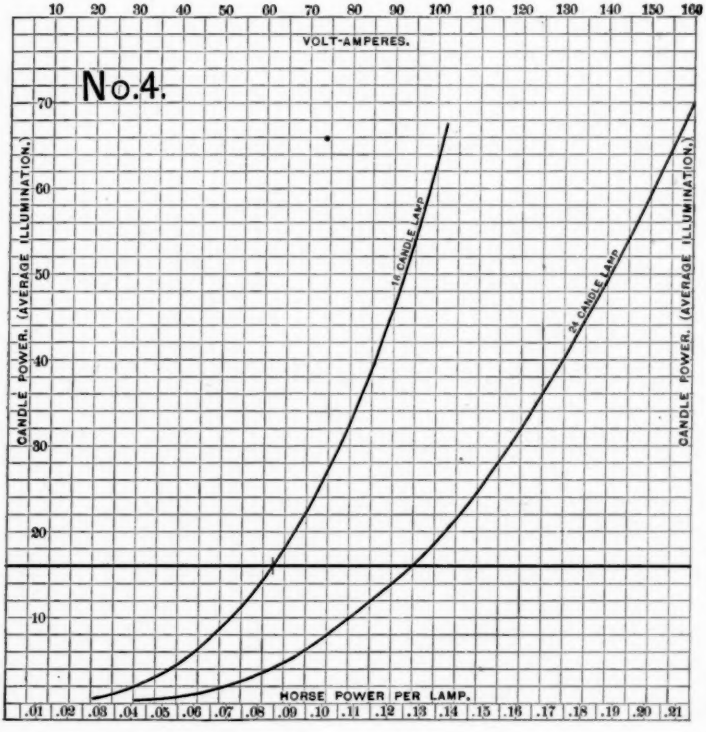
### THE PHOTOMETRIC TESTS.

In these, and all the observations with

the photometer, five or more readings were taken at every point, and their mean used as the true reading. After considerable practice, these photometric readings could be made quite exactly.

In these measurements, a Carcel lamp, burning colza oil, was used as a standard of light, the employment of a candle for this kind of work being impracticable for various reasons. In order to obtain as steady a light as possible from the Carcel, the top and bottom of the flame were diaphragmed off, and only the middle, or least variable portion, was used as a standard. The width of the aperture was adjusted so that the lamp gave an average of about ten candle power.

Though the light of the Carcel lamp was not subject to sudden fluctuations, like that of a candle, and would remain practically constant during a single series of observations, there was nevertheless a slow variation, due to the gradual crusting over of the wick, and to differences in the temperature of the room at different times. The error from these causes was eliminated, as far as concerned the variation in the candle power of each individual lamp, by taking always a series of observations in which the candle power was gradually increased, and immediately following it by another series in reverse order in which the candle power was decreased to the starting point.



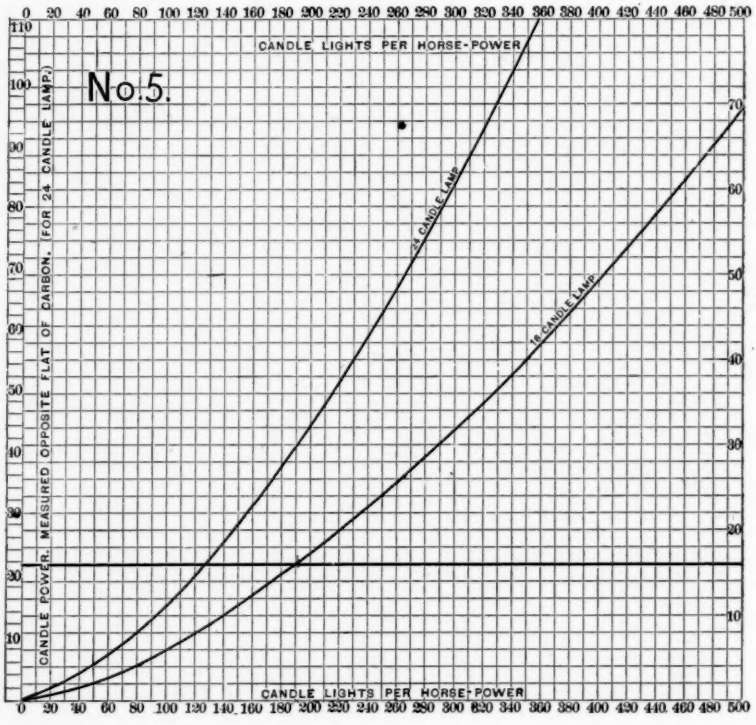

The results of these observations, then, while showing accurately the relative changes in the candle power of each lamp for given variations in electromotive force and current, could not be implicitly relied upon for comparisons between different lamps.

Accordingly, while the electrical quantities were here obtained in absolute

measure, it was not attempted in this part of the work, to measure candle power with absolute accuracy, but merely to obtain the data from which a curve could be constructed for each lamp showing on some arbitrary scale the *variation* in candle power with different electromotive forces.

The remaining problem was to reduce all these curves to the same absolute scale. One of the lamps was next run for some eight hours at about its rated candle power, and very carefully compared with a standard candle, measurements being constantly taken of the candle power, current, and electromotive force. The candle was hung on the arm of a balance made for the purpose, and the time required for the consumption of each twenty grains was noted, and the proper corrections made. Several standard candles were used, all of which were obtained from Tufts Brothers, Boston, and burned very nearly 120 grains per hour.

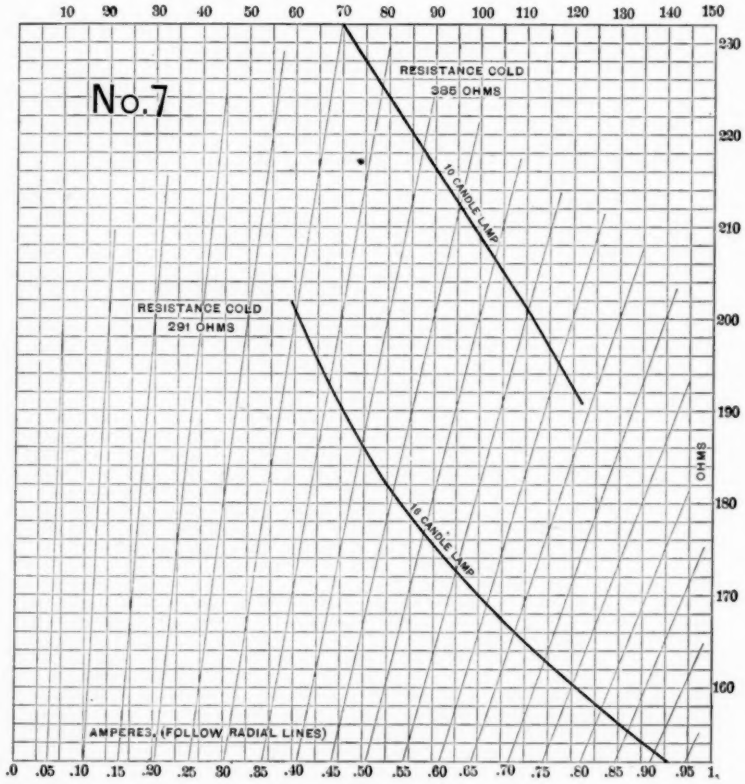
The average of the above observations gives us the absolute candle power and electromotive force for a single point on the curve of this individual lamp. To get the entire curve into absolute measure it is simply necessary to change the scale of the diagram to conform to this measurement. Having done this, we have two correct curves for a single lamp, in which abscissas represent volts, and ordinates represent, in the one curve, amperes of current, and in the other, candle powers.


No.5.
CANDLE LIGHTS PER HORSE-POWER
CANDLE POWER. MEASURED OPPOSITE FLAT OF CARBON. (FOR 24 CANDLE LAMP.)
24 CANDLE LAMP
18 CANDLE LAMP
CANDLE LIGHTS PER HORSE-POWER
0 20 40 60 80 100 120 140 160 180 200 220 240 260 280 300 320 340 360 380 400 420 440 460 480 500
110 100 90 80 70 60 50 40 30 20 10
70 60 50 40 30 20 10

### DIRECT COMPARISON OF EFFICIENCIES.

The remaining curves were next reduced to the same absolute scale by a system of direct comparisons of the lamps at opposite ends of the photometer.

The method consisted in placing at one end of the photometer bar, a lamp whose curves of candle power and current had been already obtained in absolute measure, and at the other end the lamp whose curve was to be reduced to that standard. The lamps were then run at their rated candle power, and their relative brilliancy ascertained by the photometer.




If the two lamps were of the same kind, or required about the same current strength to bring them to their normal brightness, they were connected in series, and the strength of the current through both ascertained by the ammeter. In other cases they were placed in multiple arc, adjusted to the proper relative candle power by a variable resistance in one branch of the circuit, and the current through each branch measured by the bridge method.

By thus obtaining accurately the ratio of the currents flowing through two different forms of incandescent lamps, while they are running at a given relative brilliancy, and knowing, from our previous experiments the ratio of the resistances of the two lamps with these given currents, the *relative efficiencies* of the lamps are ascertained independently of the absolute measurements.

Having obtained in this way the relative candle power of two lamps at two given points on their respective curves, it is simply a matter of altering the scale of one diagram to bring both to the same absolute standard.

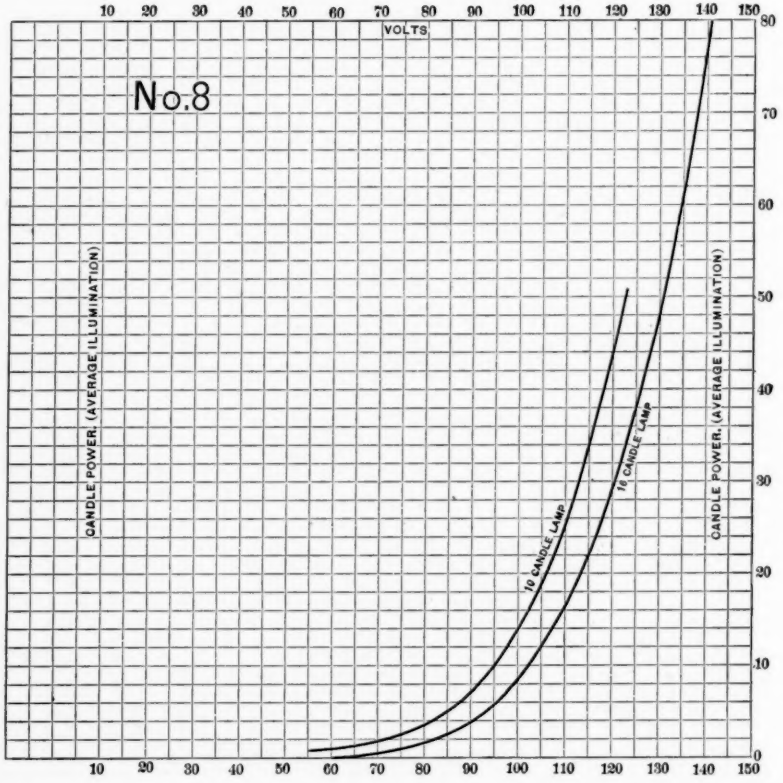
The great advantage of this comparative method lies in the fact that as but

one absolute determination of candle power is made, this one can occupy a good deal of time, and be made with great care; and further, that as the *relative* efficiencies of the different lamps are ascertained by comparing the light of one lamp directly with that of another, while at the same time the relative values of the currents and electromotive forces are obtained by direct comparison, any errors that may be made in the absolute measurements will affect all the lamps alike, and *cannot alter the relative efficiencies.*

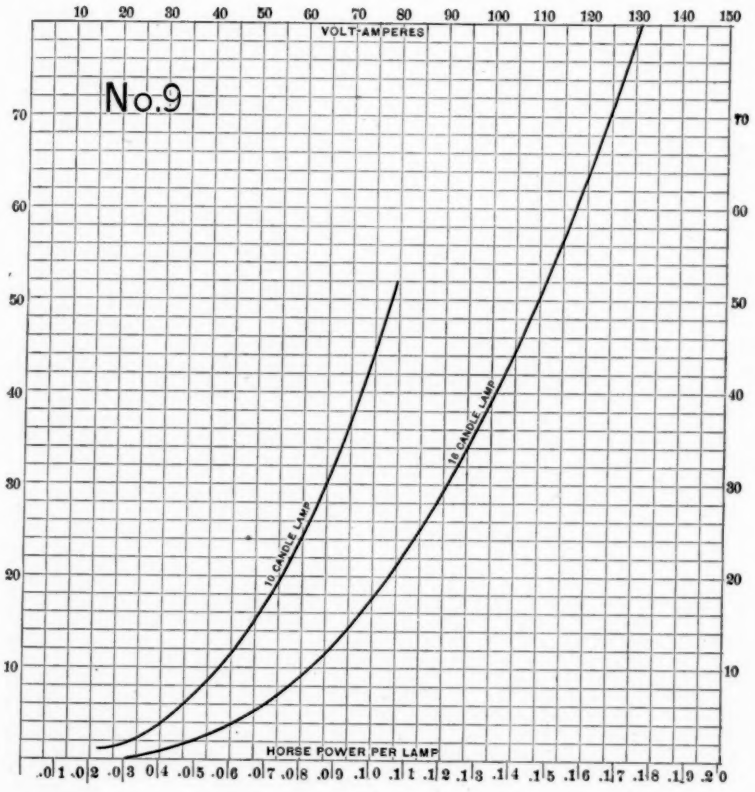
It is of course impossible in a brief memoir to describe all the details and precautions against error employed in work which extended over a period of about two months. Therefore, having given a sufficient account of the methods used to enable one to form a fair idea of their reliability, we now pass on to consider

THE RESULTS.

The first result of the experiments just described was a series of curves, showing, for each individual lamp tested, the value of the current strength in amperes, and the candle power, for a range of electro-motive forces, from the point where the carbon is barely incandescent, to a point where the lamp is giving four or five times its normal candle power. These results were carefully averaged for each class of lamps. The method of averaging was to take lamps of the same kind,—not at the same candle power, but at the same electromotive force, as they are intended to be run in practice,—and find,



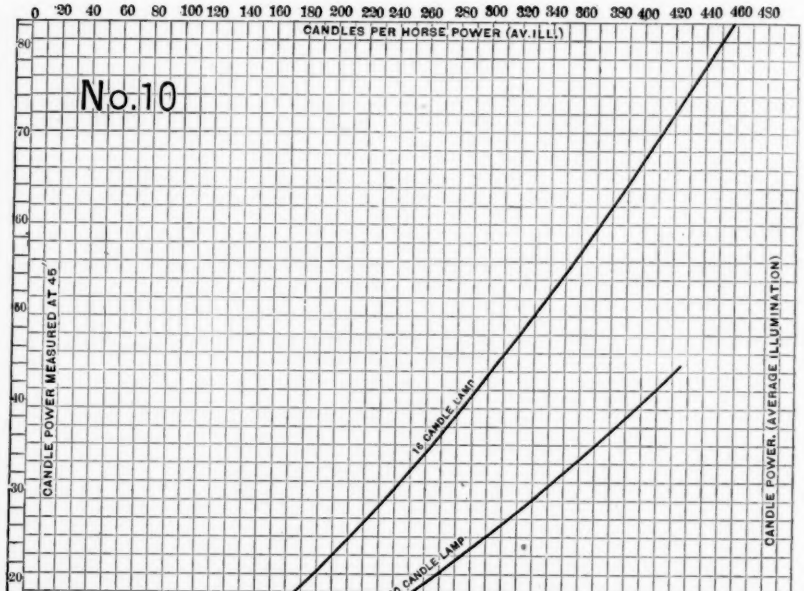
from the curves for the separate lamps, the values of the candle powers and amperes at intervals of five volts. Then the average of these values was taken at each point; the average resistances being computed by dividing the number of volts by the average number of the amperes. This gives to all the quantities the same mean values which they would have in an actual case, where the lamps are run in multiple arc.




The average results have been represented graphically, and are shown by the diagrams hereto annexed. The curves of equal illumination have already been referred to. The next curves plotted were intended to show, for each class of lamps, the relation between the electromotive force, current, and resistance. Diagram No. 2 shows these curves for the Maxim lamps, and No. 7 shows the same thing for the Edison lamps. In these diagrams electromotive force in volts is laid off on the horizontal scale at the top, and the curve for each lamp is so constructed that the height of any point on it represents on the vertical scale at the right. the resistance of the lamp in ohms, under the corresponding electromotive force. It is also desirable to show the current which will flow through the lamp under a given electromotive force, and also the resistance of the carbon when a given current is passing through it. We have, then, to express the relations of three pairs of quantities. This might be done by three separate diagrams; but as the current is always equal to the electromotive force divided by the resistance, it may be represented, for any point on

the curve, by the tangent of the angle included between the vertical axis of co-ordinates, and a line from the origin to the given point. Therefore, by drawing radial lines through the origin, (which is in this case outside the limits of the diagram,) we are enabled to lay off amperes of current on a scale of equal parts at the bottom, thus making the same curve express the relation between three variables.

tal line from that point to the scale at the right; or to find the electromotive force, follow a vertical line through the same point to the scale at the top. In short, given any one of the three quantities,—electromotive force, current, or resistance,—the other two may be found at once from a single curve. This device for expressing graphically the triple relation between the principal electrical quantities the writer believes to be new, and it is




To find the current for any point on either of the curves, it is necessary only to follow a radial line from the given point down to the scale at the base of the diagram; or, starting with a certain current, follow the radial lines up to the corresponding point on the proper curve; then to find the resistance of the lamp with the given current, follow a horizon-

presented as one that will be frequently found convenient.

In diagrams No. 3 and No. 8 electromotive force is again represented on the horizontal scales at the top and bottom, and the height of any point on either curve shows on the vertical scales at each side the number of candle powers of light

which the lamp will give with the indicated electromotive force.

The resistance of a lamp is found to fall quite rapidly as the electromotive force is increased, until the carbon becomes incandescent, when the change goes on more slowly, the resistance of a lamp when giving its light being rather more than half its resistance when cold. As will be seen from diagrams No. 3 and No. 8, the candle power increases slowly at first, but afterwards with increasing rapidity, until, if the electromotive force is carried above a certain point, the carbon filament is broken.

In diagrams No. 4 and No. 9, the horizontal distance of any point on either curve to the right of the origin represents the power consumed in the lamp, expressed in horse power on the scale at the bottom, and in volt-amperes on the scale at the top. Candle power is again laid off on a vertical scale. These curves show clearly what has before been stated, *i.e.* that a certain amount of power must be expended in a lamp before we begin to get any light; and that the brilliancy of the light expressed in candle powers then rises more and more rapidly as the energy expended is increased.

In diagrams No. 5 and No. 10 the vertical height of any point of either curve corresponds as before to the candle power of the lamp, and the distance of the same point to the right of the origin measures, on the horizontal scales at the top and bottom, the number of candle lights per horse power which lamps of this class will furnish when running at the indicated candle power. These curves show us how rapidly the efficiency of the lamps increases as the candle power is raised.

In diagrams No. 3 and No. 5 the vertical scale at one side indicates the apparent candle power as measured opposite the face of the carbon, and that at the other side the average or true candle power. It will be observed that the Maxim 16 candle lamp, at its normal candle power (indicated on the diagram by the heavy horizontal line), gives a light equal to 22 candles opposite the face of the carbon, and that the average illumination is about 73 per cent. of the face measurement.

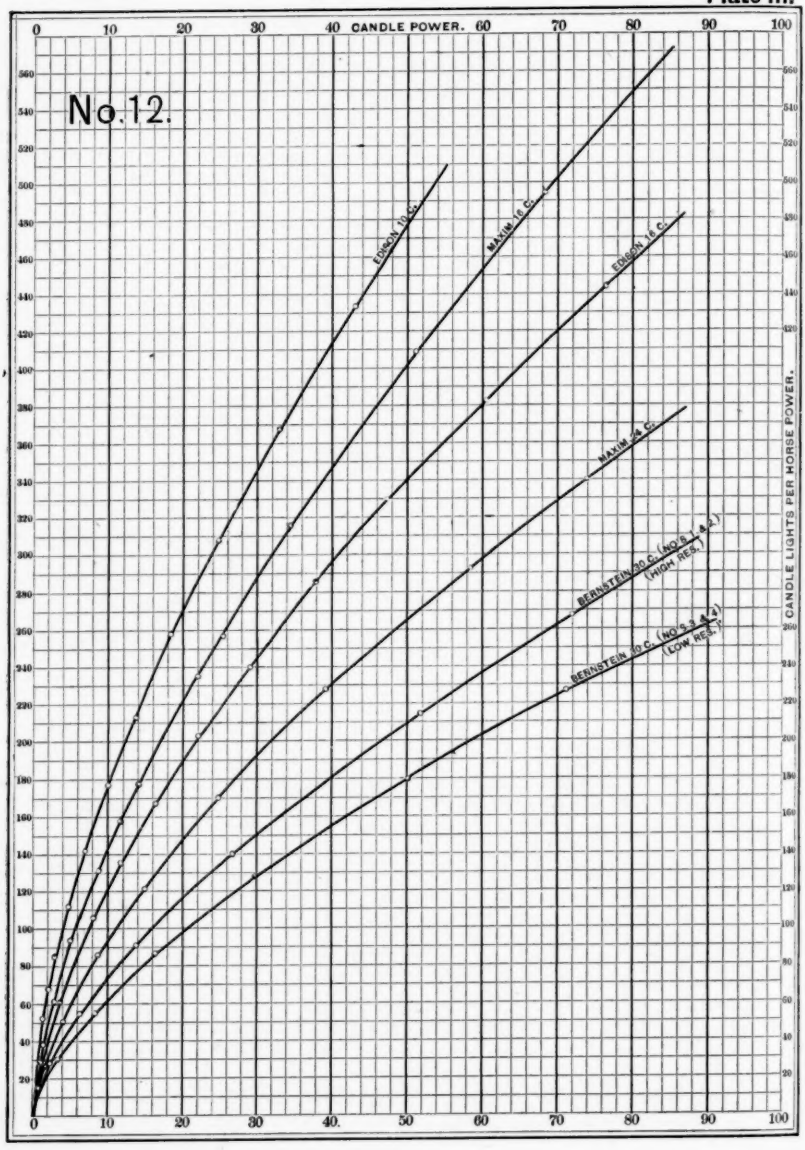
The curves in all the diagrams are drawn exactly through the plotted points; the slight irregularities apparent in the curves for the separate lamps being almost wholly eliminated in the average.

In Fig. 12 the curves representing the change in efficiency of the various lamps at different candle powers are shown upon a single diagram for the purpose of comparison. Here candle power is laid off on a horizontal scale, and, for each class of lamps, the height of any point of the curve indicates on the vertical scale the number of candle lights per horse power which the lamps will give when running at the corresponding candle power. It is noticeable that all the curves have the same characteristics, viz., that the efficiency at first rises very rapidly with increase of candle power, and that afterwards, while the change goes on more and more slowly, the rate of increase in efficiency becomes at the same time more nearly uniform, or, in other words, the curve approaches to a straight line. This would indicate, that however high the temperature of a carbon filament may be raised, raising it higher will still increase its efficiency as a light giver.

It must not be inferred, because the curve of any lamp is above the rest, that this lamp is absolutely more efficient than the others; for, as already pointed out, when a lamp designed to give 10 candle power is run at, for example, 16 candle power, its efficiency becomes higher than its normal value; whereas, if a 24 candle lamp is run at 16 candle power, its efficiency falls below its normal value. Consequently, when lamps rated at different candle powers are compared at the same candle power, the lamp with the lower rating has an advantage. The only way to arrive at a fair comparison is to take the efficiency of each lamp at a point on its curve which corresponds to the rated candle power. Comparing the lamps in this way, we find that the Maxim 16 candle lamp stands highest, and the Edison 10 candle lamp next.

It should be noticed that the Edison 16 candle lamps furnished for this test were not of the same grade as those in general use for isolated lighting. The latter may be run in parallel circuit with the 10 candle lamps, each giving its own proper candle power; whereas the Edison 16 candle lamps on which these tests were made would give only about 6

Plate III.


No.12.
CANDLE POWER.
CANDLE LIGHTS PER HORSE POWER.
EDISON 10 C.
MAXIM 16 C.
EDISON 16 C.
MAXIM 24 C.
BERNSTEIN 20 C. (NO'S 1 & 2) (HIGH RES.)
BERNSTEIN 10 C. (NO'S 3 & 4) (LOW RES.)

candle power on the same circuit with the 10 candle lamps, when the latter were at their normal brightness. By refering to diagram No. 8, it will be apparent that at the same electromotive force the 16 candle lamps gave only about two thirds as much light as the 10 candle lamps. This peculiarity was due to the high resistance of the 16 candle lamps, which, as will be seen from the

table of resistances previously given, averaged about 291 ohms when cold. Measurements afterwards made by the writer upon ten 16 candle lamps, collected from various plants of the Edison Company throughout New England, showed the following results:

RESISTANCE COLD.

| | | |
|---|---|---|
| Lamp No. | 1 | 162 Ohms. |
| " " | 2 | 176 " |
| " " | 3 | 163 " |
| " " | 4 | 186 " |
| " " | 5 | 177 " |
| " " | 6 | 175 " |
| " " | 7 | 189 " |
| " " | 8 | 186 " |
| " " | 9 | 212 " |
| " " | 10 | 197 " |
| Average resistance | | 182.3 Ohms. |

These lamps averaged 16 candle power at 95 volts, the same electromotive force required to bring the 10 candle lamps to their normal brightness, and were therefore evidently the grade of lamps intended to run on the same circuit with the latter. They were not obtained in time to make a complete test at all candle powers, but measurements were made at the rated candle power, which gave the following average results.

EDISON LAMPS AT 16 CANDLE POWER.

| | |
|---|---|
| Electromotive force | 95. volts. |
| Current strength | 0.92 ampere. |
| Resistance | 103.3 ohms. |
| Volt-amperes | 87.4 |
| Candle lights per horse power | 136.6 |
| Lamps per horse power | 8 54 |

The following are the figures for the Maxim 16 candle lamps.

MAXIM LAMPS AT 16 CANDLE POWER.

| | |
|---|---|
| Electromotive force | 68.9 volts |
| Current strength | 0.9 ampere. |
| Resistance | 76.5 ohms. |
| Volt-amperes | 62.2 |
| Candle lights per horse power | 192. |
| Lamps per horse power | 12. |

The results of these experiments indicate that the essential factor in the efficiency of an incandescent lamp is not high resistance, but high incandescence. The Maxim lamps, which displayed the highest efficiency, gave a slightly whiter light than the others, showing that the carbons were heated to a higher temperature; though their resistance was lower than that of the Edison lamps. The fact that a low resistance filament is usually coarser than one of high resistance would tend to make it endure equally well at a higher temperature.

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# TESTS OF RUTLAND AND WASHINGTON COUNTY SLATES.

By J. FRANCIS WILLIAMS, C. E., B. S.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

THE tests given in the following tables were made June 7th and 9th, 1884, in the mechanical laboratory of the Rensselair Polytechnic Institute, Troy, N. Y., on a 50,000 pound testing machine made by Tinius Olsen.

The purple and green specimens were from Fair Haven, Vt., and the red from Granville, N. Y. They were cut and smoothed by the Fair Haven Marble and Marbleized Slate Co., of Fair Haven, Vt.

Before making each test, the piece under consideration was carefully measured on a micrometer measuring apparatus, and the size in each direction recorded. The results were recorded as pressures per sq. inch. The pieces experimented upon did not differ more than 2 or 3 hundredths of an inch in a normal section.

The experiments marked "compression" were made upon blocks about one square inch in section and $1\frac{1}{2}$ inches in height. In the cases marked "wood" blocks of wood $1\frac{1}{2}\times1\frac{1}{2}\times1\frac{1}{4}$ inches were used as cushions, one above and one below the specimen, they were of soft wood and were so placed that the grain did not run in the same direction in the two pieces, and thus there was no tendency of the blocks to become crooked in the machine.

With those marked "paste-board," book-binder's board was used in pieces of $1\frac{1}{2}''\times1\frac{1}{2}''\times\frac{3}{16}''$. This is closer in texture and gave results about one-fifth larger

TESTS.

| Substances used as Cushions and Methods of the Tests. | Purple. Weight ℔ sq. in. | Purple. Remarks. | Red. Weight ℔ sq. in. | Red. Remarks. | Green. Weight ℔ sq. in. | Green. Remarks. |
|---|---|---|---|---|---|---|
| Compressive wood..... | 17330 | 1 seam | 14390 | crack 11220 | 13760 | |
| " " ..... | 22140 | | 13535 | 1 seam | 12520 | 2 seams |
| " " ..... | 20540 | crack 16010 | 14580 | | | |
| " " ..... | 18700 | 2 seams | | | | |
| " " ..... | 18170 | crack 17370 1 seam | | | | |
| " paste-board. | 24760 | | 14590 | crack'd at 12440 1 seam | 17420 | 2 seams |
| " " | | | 18160 | | 17930 | 1 seam |
| " " | | | 21310 | | 16840 | 1 " |
| " " | | | 18380 | | 18060 | crushed at op. angles |
| " no cushion | 12640 | | 10190 | | 8040 | 2 seams |
| " elastic limit | 9860 | | 4850 | | 5150 | |
| " no cushion. | 13430 | | | | | |
| " elastic limit | 10480 | | | | | |
| " no cushion. | 15530 | small seam | | | | |
| " elastic limit | 10440 | | | | | |
| " on side-wood. | 15195 | Section 1" × 1½" 2 seams | 24030 | Section 1" × 1½" | 17760 | crack 16700 1 seam |

TESTS—Continued.

| Method of Test and Length of Beam. | Purple. Weight. Center Load. | Purple. Number and Remarks. | Red. Weight. Center Load. | Red. Number and Remarks. | Green. Weight. Center Load. | Green. Number and Remarks. |
|---|---|---|---|---|---|---|
| Bending 6 in. span..... | 1150 | (1.) | 775 | (1.) 1 seam | 950 | (1.) |
| " " ..... | 1025 | (2.) split along | 1100 | (2.) | 775 | (2.) small seam |
| " " ..... | 1425 | (3.) | 450 | (3.) bad seam | 1025 | (3.) |
| " 3 in. span..... | 2400 | (4.) | 1875 | (2.) no seam | 1825 | (2.) |
| " " ..... | | | 2250 | (2.) | 2400 | (3.) |
| " " ..... | | | 1000 | (1.) 1 seam | | |
| Compression 10 in. column. | 20000 | | 17730 | | 16020 | |

RESULTS OF THE ABOVE TESTS.

| | | | | | | |
|---|---|---|---|---|---|---|
| Compression, wood..... | 19376 | No. of specs. 5. | 14170 | No. of specs. 3. | 13140 | No. of specs. 3. |
| " paste-board:.. | 24760 | " 1. | 18110 | " 4. | 17560 | " 4. |
| " no cushions.. | 13860 | " 3. | 10190 | " 1. | 8040 | " 1. |
| " elastic limit.. | 10260 | " 3. | 4850 | " 1. | 5150 | " 1. |
| Bending 6in. span..... | 1200 | " 3. | 770 | " 3. | 910 | " 3. |
| " 3in. " ..... | 2400 | " 1. | 1710 | " 3. | 2110 | " 2. |
| Compression, long columns. | 20000 | " 1. | 17730 | " 1. | 16020 | " 1. |
| " on side ..... | 15195 | " 1. | 24030 | " 1. | 17760 | " 1. |

than the wood, and I should recommend that all experiments with stone be made with such cushions.

In the preceding cases the blocks have all been placed so that the cleavage planes of the slate are horizontal—perpendicular to direction of stress—but in the case marked "on side" they were

placed in a vertical position and the specimen then had an area of section of $1\frac{1}{2}$ square inches. This was not reduced to inch square.

Those tests marked "no cushion" were made on specimens which had particularly smooth and parallel upper and lower surfaces, and the elastic limit was carefully observed. The U. R. in this case of course was low, owing to the unequal distribution of pressure over the surfaces.

The beams tested were an inch in section and 10 inches long. The supports were placed 6 inches apart and were covered with paste-board so as not to cut the extreme fibers of the beam. The point in the center, by which the pressure was applied was also covered with paste-board.

After the beam was broken with a six inch span the two portions were in some cases broken again as beams of a three inch span. This is indicated by the numbers placed in brackets after the breaking weight. The first three being numbered consecutively, and those which are used a second time being designated by the same numbers that they had when first broken. These, of course, were broken with their cleavage planes horizontal.

The last specimen was broken as a long column with the cleavage planes vertical. It stood more than the small blocks, crushed and broke with considerable violence.

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## A NOTE ON THE LONG COLUMN QUESTION.

By WM. H. BURR.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

It is not a matter of surprise that Prof. Robinson should take exception to my criticism of his long column formula, or that Mr. Weston should desire to give to the immense amount of labor, involved in the computation of his tables, a character other than that of wreckage. After the lapse of so much time, one would suppose that some new point of demonstration would have been developed, or, at least, that something tangible would have been said. It is also a little remarkable that Prof. Robinson makes no attempt to strengthen the points in his analysis which I attacked. Hence, it may be supposed that he considers them hopelessly overthrown—one point, at least, on which we agree. I had also hoped that he would render a lucid explanation of the fact that his "rational" formula gives a curve convex upward within the limits, for which experiments give a curve *concave* upward. (See Magazine for Nov. 1883.) But as he omits the explanation and stoutly maintains that his formula is certainly, truly, and incontestably "rational," we are left to infer that the results of the tests are unquestionably irrational, and that the upward concavity of the curve is simply and only an evidence of the original and "total depravity of inanimate things" which has from time to time been hitherto observed under proper circumstances.

But in order to come to a more detailed consideration of Prof. Robinson's last article, his analytical positions will first be examined while subsequent attention will be applied to experimental matters.

In the first place he proposes to give a new demonstration of his formula without the slightest reference to Euler's, by considering the neutral curve of the column a sinusoid!!!

As a result, in the April (1884) number of this Magazine, page 184, he finds the following value of the radius of curvation:

$$\rho_1 = \frac{l^2}{\pi^2 y_1} \quad . \quad . \quad . \quad (d).$$

Eq. (*d*), it is to be particularly observed, is supposed to be very different from Euler's formula.

Now, in a little book published in 1882, "Strength of Wrought Iron Bridge Members, by S. W. Robinson, C. E.," "an authority which my opponent will doubtless accept as sound," on page 84 is found Euler's formula:

$$\frac{T}{EI_1} = \frac{\pi^2}{l_1} \quad . \quad . \quad . \quad (1);$$

and on page 93, the usual equation of the common theory of flexure;

$$\frac{EI}{\rho} = Ty_1 \quad . \quad . \quad (2)$$

The subscript 1 has been introduced in eqs. (1) and (2), in order to make the notation uniform with eq. (*d*).

If the value of $\rho_1$ be taken from eq. (2) and inserted in eq. (*d*), then will result:

$$\frac{T}{EI_1} = \frac{\pi^2}{l^2};$$

which is Euler's formula, and identical with eq. (*d*). In reality, therefore, Prof. Robinson has given in his sinusoidal method simply "a new toot on an old horn." Can it be possible that he was ignorant of the fact, that the use of the sinusoid simply meant the use of the common theory of flexure in another *form*, only, than Euler's formula?

Since his "new demonstration" is thus seen to involve Euler's expression for the resistance of a long column, another "new demonstration" from the same source is now in order, and it will be awaited with interest.

After having vainly endeavored to shut Euler's formula out of view, by heaping over it considerable unnecessary sinusoidal analysis, Prof. Robinson then subjects the "classical" formula

$$M = \frac{RI}{d_1} = \frac{RAk^2}{d_1}; \quad . \quad . \quad (3)$$

to some most unhandsome treatment. We are told, on page 285, that R is the modulus of rupture for bending and that, hence, a beam will sustain the least load in that plane in which $d_1$ is greatest. Now I am aware that Prof. Robinson is not alone in that impression, though it is most erroneous. As a matter of fact, eq (3) is an equation deduced in the mathematical theory of elasticity in solid bodies, and has no "rational" connection whatever with the *failure* of beams or material. It is based upon the hypothesis of perfect elasticity, and is merely another form of $\frac{EI}{\rho}$, $R \div d_1$ representing $E \div \rho$ or the intensity of stress at unit's distance from the neutral surface.

When R is given the signification of the "modulus of resistance" and determined by experiments on the failure of beams, it ceases to have any rational or analytical meaning, though it is thereby made of inestimable value to engineering practice. Prof. Robinson handles this part of the matter with that freshness and *naiveté* which is so characteristic of unconcious lack of knowledge, and it is useless to carry this part of the discussion any farther until he informs himself more thoroughly on some of the more fundamental portions of the theory of flexure. I would refer him to C'ebsch's "Die Theorie der Elasticität Festr Körper," where he will find an admirable treatment of the subject.

Prof. Robinson's reference to the Watertown tests of Phœnix columns scarcely agrees with the facts as given in the copy of the Trans. of the Am. Soc. C. E. now before me. As nearly as can be learned from that publication, Nos. 1, 4, 5, 7, 8, 12, 13, 16 failed in the plane of two flanges, while Nos. 2, 6, 9, 10, 11, 14, 15, 17 gave way in the direction of the barrel between the flanges—an equal number in each direction—while the remaining 6 cases are doubtful. These results therefore show that a Phœnix column is equally liable to fail in any direction—a conclusion which has been verified by other experiments at Phœnixville as well as by a proper consideration of the theory. Prof. Robinson's error lies in considering R as separate from $1 \div d_1$; he should consider $R \div d_1$ as a single quantity.

The old paradox of the triangular beam may be properly used by a youthful instructor to impress his first class with the mysteries of the resistance of materials, but it is good for nothing else. Such a beam is just as strong as, but no stronger than, the trapezoidal one. Its fibers begin to rupture at an earlier stage of loading, but the ultimate load held, is exactly the same in both cases.

We now come to the experimental portion of the April paper, and we find in looking over Prof. Robinson's application of his "rational" formula that he causes T to vary from 40,000 to 45,000, and E from about 23,000,000 to 31,000,000, in order to get a satisfactory agreement between his results and those of experiment. Now this is precisely what is done in all empirical formulas, and its necessity might be a matter of surprise in the present instance if I had not shown in

my first paper that his formula is simply and only a mongrel form of Gordon's; hence he is obliged to doctor it in passing from one form of section to another, in order to get a satisfactory approach to agreement with the results of tests.

But there are some features of his comparison of Gordon's formula with his own which ought not to be overlooked.

In the first place, while he gives to the empirical factors in his own formula such values as are needed to reach the closest agreement to experiment, he takes an antiquated form of Gordon's formula with empirical quantities which belong to a solid rectangular section in iron produced forty years ago, or more, and applies it, without change, to all forms of sections. Such a suspicious procedure can only result from either ignorance or design, and I refrain from comment.

But even with such aid the mean discrepancies of his own formula are less than those of Gordon's by only 400 or 500 pounds at most,—an insignificant amount for such a matter. In fact a truthful presentation of the table at top of page 288 of the April No. would probably turn the balance in favor of Gordon's formula. In that table there is a column headed "New Formula," with the evident design of conveying the impression that the results of that column come from his own formula, though an incidental explanation is given in the text. The upper five values of the column in question (nearly one-third of the whole) are the results of computation by Euler's formula, and *not* by Robinson's. To properly name such an action under the present circumstances would carry me beyond the limits of propriety of such a discussion, and I simply give the upper part of the table as it ought to have appeared.

| $l \div k$. | Experiment. | New Formula. | Rankine's or Gordon's Form. |
|---|---|---|---|
| 643 | 2410 | 5130 | 2885 |
| 540 | 3380 | 6140 | 3956 |
| 414 | 4280 | 8050 | 6250 |
| 400 | 5630 | 8350 | 6604 |
| 311 | 9600 | 10800 | 9921 |
| 300 | 9750 | 11200 | 10290 |

His reason, he says, for giving $t$ the remarkably high value of 45,000 is that the iron was "solid," "flat" and "English"!! Such a grade of metal must truly possess unusual characteristics.

Reviewing the whole field, then, it appears that by dint of fitting different empirical factors to Prof. Robinson's "rational" formula, for different forms of section, results may be obtained which, on the whole, are little or no better than those arising from an unfair and improper application of an antiquated form of Gordon's formula.

In regard to Mr. Weston's paper, little need now be said. His analytical attempt to fix the safe limit of deflection is simply a futile effort to distort an equality into an inequality, and is precisely the same feature which I attacked in Prof. Robinson's original paper; and as he has since made no attempt to repair the damage, it is scarcely worth while to repeat the criticism.

Mr. Weston's appeal to the theory of flexure comes within the scope of my observations on Prof. Robinson's attempts in the same direction; and his sublime faith in the innate superiority of the new formula to the results of experiment, makes it rather useless to attempt to bring him down to a consideration of a set of experimental results. Since, in fact, he thinks (pp. 401 and 402 of May No.) that "the new formula cannot, with justice, be compared with experimental results obtained in the usual method of breaking columns," and that "it is a question whether practice would really desire the new formulas to agree with the results of experiments for ultimate strength;" also, that the "irregular agreement of experimental and computed results, when $t$ is given a value approximately equal to the ultimate resistance, does not prove or disprove the new formula, but, on the other hand, rather weakens the confidence in the usefulness of experimental results for ultimate strength;" it becomes a serious question whether he is not only using rather a "free lance" against engineering column practice, but also taking Prof. Robinson both in flank and rear for making such desperate and questionable efforts to get even an approximate agreement between the results of tests and his own formula. I leave them to settle this question between themselves and take my adieu of the discussion.

# THE EVAPORATIVE POWER OF ANTHRACITE COAL IN TESTS OF STEAM BOILERS.

By WILLIAM KENT, M. E.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

That portion of Mr. R. H. Buel's paper on the Compound Steam Engine, etc., which relates to the Steam Boiler (VAN NOSTRAND'S ENGINEERING MAGAZINE for June) contains so many statements which are at variance with the generally received opinions concerning boiler performance and economy, that it should not be allowed to become a portion of American engineering literature unchallenged. It would be difficult to discuss completely such an elaborate paper, crowded as it is with tables of figures, without occupying more space than is taken by the paper itself, but a few of the statements to which exception may be taken may be discussed briefly.

Mr. Buel states, in reference to the summary of the experiments of Messrs. Kestner and Meunier: "There are no American coals in this table, but as the best anthracite coal of the United States contains more fixed carbon than any specimen of Table XI., it is reasonable to suppose that it is at least equal in heating power to the best European coal tested by Messrs. Kestner and Meunier."

It is unreasonable to make the supposition, 1st, because there are no records anywhere published of accurate determination of the total calorific value of American anthracite by which to support the supposition. 2d, because in Kestner and Meunier's table, the Groufcheski anthracite, which contains the largest percentage of fixed carbon of any of the coals, has the lowest evaporative power but one (lignites excepted), and hence by analogy we would infer that the American anthracites having higher percentages of fixed carbon, would have lower evaporative power (per pound of combustible). 3d, because, as stated by Mr. Buel in his quotation from Gruner, "the heat of combustion of a body, simple or compound, is in general greater in proportion as its molecular condensation is less advanced," and "the heat of combustion like the specific heat varies with the density." Moreover, as shown by M. Gruner, since the diamond gives by experiment only 13,986 units, graphite 14,035, gas retort charcoal 14,485, the less dense wood charcoal 14,544, and gaseous carbon theoretically 20,186 heat units, the calorific power increasing as compactness of structure diminishes, it would be reasonable to suppose that anthracite, in which the carbon is probably as compact as in gas retort charcoal, would have a similar heating value, or about 14,500 heat units.

The writer has always in his experience in testing boilers, or in calculating the results of tests, assumed the calorific value of the combustible portion of anthracite coal to be 14,500 heat units, equivalent to an evaporation of 15 lbs. of water from and at 212°; and he has thus far seen no reason to believe that more than 80 per cent. of this heating power can be utilized under ordinary working conditions in any boiler in which the products of combustion are allowed to escape at a temperature above 350° or 400°.

Mr. Buel gives a number of results of experiments "generally regarded as being amongst the most reliable that have ever been made, all of which must be repudiated by those who believe that the maximum heating effect attainable from the combustible in the best varieties of American anthracite is 14,544 thermal units per lb., and that the average amount of air supplied to furnaces consuming American anthracite is from 25 to 30 lbs. per lb. of coal."

In the absence of any direct and conclusive evidence to the contrary, the reasons given above are sufficient for believing that the heating value of the combustible in the best American anthracite is not more than 14,544 thermal units. And any test of a steam boiler (made as ordinary boiler tests are made, without either measuring the quantity of air supply, or analyzing the coal or the products of combustion, or making direct

test of the heat lost by radiation) which shows a result which would require the heating value of anthracite to be greater than 14,544 units, must be repudiated on that ground alone, if there were no other, notwithstanding the weight of authority which is presumably due to the experiments of Isherwood, or any other engineer "generally regarded as being among the most reliable."

As to the "average amount of air supplied to furnaces consuming American anthracite" being from 25 to 30 lbs. per lb. of coal, perhaps there are not enough data in existence from which to form such an average. In the six anthracite tests by Johnson in Mr. Buel's Table XII., the amount of air supplied ranges from 18.4 to 47.9 lbs., the average being 26 lbs. In the comparative test between Babcock & Wilcox and return tubular boilers, published in VAN NOSTRAND'S MAGAZINE in 1883, Mr. Hoadley found the weight of flue gases per pound of combustible to be respectively 30.71 and 32.10 lbs. Until direct experiment and analysis prove the contrary there seems to be no reason to suppose that the average amount of air supplied in burning a pound of anthracite coal is less than 25 lbs.

Let us suppose that the smallest quantity of air which will burn the carbon of the coal completely is supplied, viz., 12 lbs., or that the flue gases weigh 13 lbs. per lb. of combustible, and that the maximum probable calorific value of the combustible portion of anthracite is 14,544 heat units, and apply these two assumptions to experiments Nos. 5 and 9, Table XXV and No. 1, Table XXII of Mr. Buel's paper.

The results shown in this table lead to the conclusion either that the heating value of anthracite coal is greater than 14,544 heat units per lb. of combustible, or that the results of tests showing evaporations of 14.86, 13.32, and 13.31 lbs. are erroneous, the errors being due to neglect of determination of priming, or to possible inaccuracy of measurement.

That the first of these conclusions is untenable has already been shown, that the second is not unwarranted may be shown, not only by the necessity which follows from the exclusion of the first, but by an entirely independent line of argument.

I have selected from Mr. Buel's tables all the figures showing rate of evaporation from and at 212° per square foot of heating surface and per pound of combustible, and arranged them in the table beneath. Column II. contains, in addition to the tests given by Mr. Buel, a few (Nos. 10 to 13) obtained from other sources, and column III. contains some figures from the results of tests at the American Institute Fair, for comparison. I have plotted these figures on cross-section paper, using the lbs. per square foot of heating surface as abscissas, and the lbs. water per lb. of combustible as ordinates, and find that a large majority of all the tests are contained within a somewhat limited field, while a few, including

| | Vertical Water-Tube Boiler (Isherwood) Table XXV. | | Stead Boiler Table XXII. |
|---|---|---|---|
| | No. 5. | No. 9. | |
| 1. Thermal units per lb. combustible | 14,544 | 14,544 | 14,544 |
| 2. Weight of flue gases per lb. combustible | .13 | 13 | 13 |
| 3. Specific heat of flue gases | .238 | .238 | .238 |
| 4. Temperature of flue gases | 322° | 600° | 621° |
| 5. Temperature of fire-room, say | 91 | 68 | 68 |
| 6. Thermal units lost in flue gases | 715 | 1646 | 1711 |
| 7. Loss by radiation, probably not less than 4 per cent. of 14,544 | 582 | 582 | 582 |
| 8. Thermal units available for evaporation | 13,247 | 12,316 | 12,251 |
| 9. Assuming above data: Maximum possible evaporation from and at 212° per lb. combustible..lb. | 13.71 | 12.75 | 12.68 |
| 10. Result of test | 14.86 | 13.32 | 13.31 |
| 11. Maximum possible efficiency per cent | 91.1 | 84.7 | 84.2 |
| 12. Apparent efficiency obtained in test | 98.7 | 88.4 | 88.4 |
| 13. Apparent efficiency greater than maximum possible per cent | 8.4 | 4.5 | 6.0 |
| 14. If we assume that the flue gases weigh 25 lbs. per lb. of combustible, maximum possible evaporation would be | 13.13 | 11.18 | 11.05 |
| 15. Apparent efficiency greater than maximum possible per cent | 13.2 | 19.1 | 20.5 |

COMPARATIVE RATES OF EVAPORATION AND ECONOMICAL PERFORMANCE.

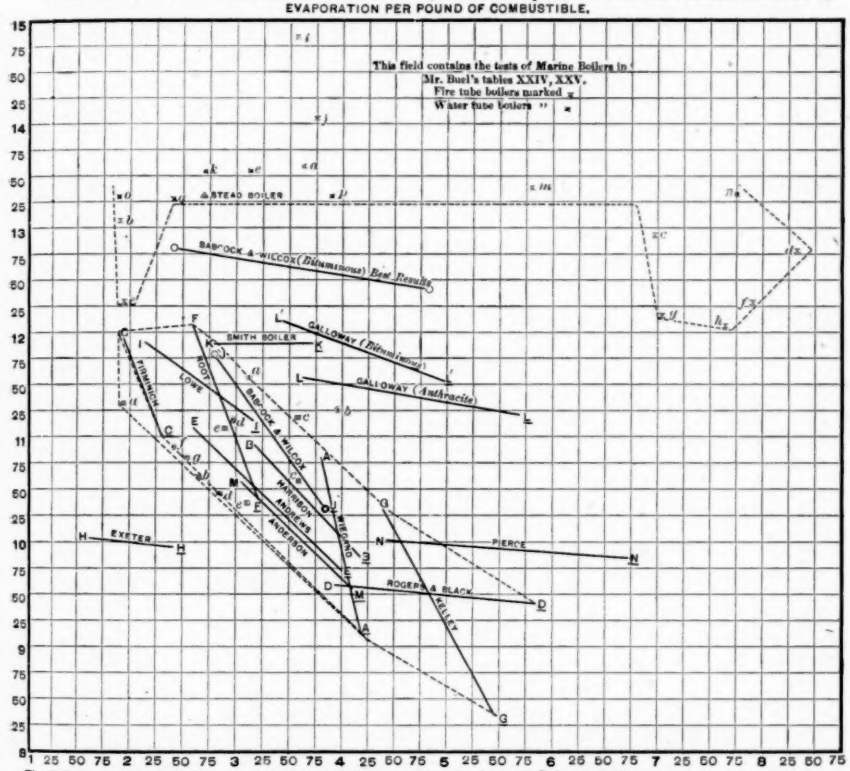
| I. Centennial Tests. Water evaporated from and at 212°. | | | II. Babcock & Wilcox Boilers. Water evaporated from and at 212°. | | | III. Tests at American Institute Fair, 1871. Water evaporated from and at 212°. | | | IV.—Tests of Cylindrical Tubular Boilers. Table XXIII. Water evaporated from and at 212°. | | | V.—Water Tube, Fire Tube and Cylinder Boilers. Table XXII. Water evaporated from and at 212°. | | | VI.—Horizontal Fire Tube Marine Boiler. Table XXIV. Water evaporated from and at 212°. | | | VII.—Vertical Water Tube Boiler. Table XXV. Water evaporated from and at 212°. | | | VIII.—Fire Tube and Water Tube Boilers. Table XXVI. Water evaporated from and at 212°. | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Boiler. | Persq.ft. heating surface. | Per lb. combustible. | Number | Pr. sq.ft. heating surface. | Per lb. combustible | Name. | Pr. sq.ft. heating surface. | Per lb. combustible. | Number | Pr. sq.ft. heating surface. | Per lb. combustible. | Number | Pr. sq.ft. heating surface. | Per lb. combustible. | Number | Pr. sq.ft. heating surface. | Per lb. combustible. | Number | Pr. sq.ft. heating surface. | Per lb. combustible. | Number | Pr. sq.ft. heating surface. | Per lb. combustible. |
| | | | 13 | 2.44 | 12.80 | | | | 2 | 1.98 | 12.92 | | | | | | | 5 | 3.58 | 14 86*i* | | | |
| L′ | 3.68 | 12.125 | 10 | 4.81 | 12.42 | | | | | | | | | | | | | | | | | | |
| F | 2.59 | 12.094 | | | | | | | | | | | | | | | | 6 | 3 75 | 14.06*j* | | | |
| C | 1.93 | 11.988 | | | | | | | | | | | | | 3 | 3.63 | 13.60*a* | 3 | 2.71 | 13.55*k* | 3 | 2.71 | 13.55 |
| *K* | 3.74 | 11.925 | | | | | | | | | | 1 | 2.69 | 13 31 | | | | 4 | 3.13 | 13 55*l* | | | |
| I | 2.15 | 11 923 | | | | | | | | | | 4 | 1.8 | 13.29 | | | | 8 | 5 79 | 13.39*m* | | | |
| K | 2.79 | 11.906 | | | | | | | | | | | | | 1 | 1.85 | 13.08*b* | 9 | 7.75 | 13.32*n* | | | |
| J | 2.79 | 11 822 | 3 | 2.79 | 11.82 | | | | | | | 9 | 3.31 | 12.17 | 5 | 6.96 | 12.92*c* | 1 | 1.88 | 13.31*o* | | | |
| *L′* | 5.06 | 11.609 | 5 | 3.11 | 11.58*a* | Blanchard | 1.92 | 11.34*a* | | | | | | | 9 | 8.32 | 12.77*d* | 7 | 3.89 | 13.30*p* | | | |
| L | 3.63 | 11.583 | 7 | 3.98 | 11.25*b* | | | | | | | | | | 2 | 1.96 | 12.29*e* | 2 | 2.40 | 13.28*q* | | | |
| *L* | 5.41 | 11.216 | 6 | 3.57 | 11.21*c* | | | | | | | | | | 8 | 7.91 | 12 28*f* | | | | | | |
| *I* | 3.17 | 11.163 | 12 | 2.98 | 11.14*d* | | | | | | | | | | 6 | 7.02 | 12.17*g* | | | | 1 | 3.43 | 12.71 |
| *C* | 2.29 | 11.064 | 4 | 2.90 | 11.10*e* | | | | | | | | | | 7 | 7.64 | 12 05*l* | | | | | | |
| E | 2.66 | 11.039 | | | | | | | | | | | | | | | | | | | | | |
| B | 3.16 | 10.930 | 1 | 2.40 | 10.91*f* | | | | | | | | | | | | | | | | | | |
| A | 3.80 | 10.834 | 2 | 2.54 | 10.81*g* | | | | 4 | 2.13 | 10.68 | | | | | | | | | | | | |
| M | 3.03 | 10.618 | | | | Root | 2.65 | 10.64*b* | 7 | 5.27 | 10.57 | | | | | | | | | | 5 | 6.12 | 9.08 |
| *F* | 3.21 | 10.441 | | | | Allen | 3 59 | 10.60*c* | 1 | 0.99 | 10.52 | | | | | | | | | | | | |
| *J* | 3.84 | 10.330 | 8 | 3.84 | 10.33 | Phleger | 2.83 | 10.49*d* | | | | 6 | 2.95 | 10.71 | | | | | | | | | |
| G | 4.40 | 10.312 | | | | Lowe | 3.10 | 10.40*e* | | | | | | | | | | | | | | | |
| H | 1.59 | 10.041 | | | | | | | | | | | | | | | | | | | | | |
| N | 4.44 | 10 021 | | | | | | | | | | | | | | | | | | | | | |
| *H* | 2.38 | 9.974 | | | | | | | | | | | | | | | | | | | | | |
| *B* | 4.15 | 9.889 | | | | | | | | | | | | | | | | | | | | | |
| *N* | 6.70 | 9.865 | | | | | | | | | | 3 | 3.76 | 9.6 | | | | | | | 2 | 10.01 | 8.24 |
| *E* | 4.00 | 9.745 | | | | | | | 3 | 1.84 | 9.60 | 7 | 4.55 | 9.13 | | | | | | | 4 | 11 80 | 8.14 |
| D | 3.94 | 9.613 | | | | | | | | | | | | | | | | | | | | | |
| *M* | 4.11 | 9.568 | | | | | | | | | | | | | | | | | | | 6 | 16.08 | 7 9 |
| *D* | 5.80 | 9 429 | | | | | | | 6 | 3.46 | 9.21 | 5 | 7.51 | 8 64 | | | | | | | | | |
| *A* | 4.19 | 9.145 | | | | | | | 5 | 1.94 | 9.13 | | | | | | | | | | | | |
| G | 5.43 | 8.397 | 9 | 4.02 | 9.05 | | | | | | | | | | | | | | | | | | |
| | | | 11 | 1.91 | 8.77 | | | | | | | | | | | | | | | | | | |

all the tests of column VII., nearly all those of column VI., but only a few of the others, were outside of the field. Some of the tests lay so far out of the field as to indicate some abnormal condition or some error. As there is nothing in the construction of the boilers to explain the positions of these tests, they must be due to exceptional qualities of coal, or to errors of test. In the tests in columns VI. and VIII. (Isherwood's) there are almost certainly errors due to primage, and they are unreliable from the fact that no priming tests were made, if for nothing else.

The figures in the tables herewith are arranged so as to show in some measure the same facts which the plotted diagram shows more clearly.

Consider column I. the Centennial Tests. (The italic letters are the capacity tests.) There were fourteen boilers, erected especially for test and competition. The same coal was used in the tests, and the same experimenter made them. The boilers were new and in excellent condition. It is to be expected therefore that every effort was made to secure the highest possible results. As is the case in every competition between strong rivals, in good condition for a race, several of the boilers came so close together in the front rank that it is difficult to distinguish between them. The five boilers at the top of the list which were tested with anthracite coal gave results ranging only between 12.094 and 11.822. These results are so close as to be within the limit of accidental variation, and are certainly

BOILER TESTS WITH ANTHRACITE COAL. RATIO OF EVAPORATION PER SQ. FOOT OF HEATING SURFACE PER HOUR TO EVAPORATION PER POUND OF COMBUSTIBLE.




The field enclosed by dotted lines contains 22 out of 28 Centennial tests. 8 out of 9 Babcock & Wilcox, marked × 5 all Am. Inst. Fair tests, marked = Centennial economy test, marked A. B., &c. Centennial capacity test, marked △ B., &c.

within the limit of error of the calorimeter tests, which have been considered erroneous by many good authorities. Rejecting the test with bituminous coal, *L'*, there follow five tests with results between 11 and 11.6 lbs., 8 between 10 and 11 lbs., and 8 between 9 and 10 lbs. With but few exceptions the tendency of the figures is towards decrease of economy with increase of rate of evaporation per square foot of heating surface. If we leave out the Galloway test *L*, the five highest boilers show an average rate of evaporation of 2.66 lbs. per square foot of heating surface, the next four 2.93 lbs., the next eight 3.43, and the last eight 4.41 lbs., which figures would indicate that if the rate of evaporation in the boilers which evaporated less than 11 lbs. of water per lb. combustible had been decreased, the economy of many of them might have been increased so as to bring them into the front rank.

The chief lesson to be learned from the Centennial tests, however, in this connection, is the fact that notwithstanding the great difference in construction of the boilers, the selection of the coal, and the generally favorable conditions of the tests, the highest result obtained by any of the boilers with anthracite coal is only 12.094 lbs. of water per lb. combustible. In a similar trial of five boilers at the American Institute Fair in 1871, column III., the best result is 11.34 lbs. The best result yet published of the Babcock & Wilcox boilers is 11.822 lbs., or, rejecting the calorimeter tests as erroneous, 12.131 lbs. Surely these figures are sufficient to render it extremely probable that higher results than 12.131 lbs. per pound of combustible are not to be expected from such anthracite coal as was used in the Centennial tests, under similar conditions of pressure of steam and temperature of flue, and if any test shows a higher result, it is *prima facie* evidence either that the coal has a higher calorific power than the coal used in the Centennial tests, or else that the test is erroneous. The fact that the Babcock & Wilcox tests have been made with many different kinds of anthracite, and none of them have given better results than were shown in the Centennial tests is also an indication that there is no better anthracite in the market than that used at the Centennial.

Assuming what is probably true, that 80 per cent. of the total heating power of a coal can be obtained in a boiler working with high pressure steam and natural draft, and that the remaining 20 per cent. covers all losses from heat carried away in the gases of combustion, radiation, evaporation of moisture in coal, etc., and that this 80 per cent. was reached in the five Centennial tests showing the best result, say 12 lbs., then the heating power of the combustible portion of the coal is equal to an evaporation of $12 \div \frac{80}{100} = 15.0$ lbs., and $15 \times 965.7 = 14{,}485$ thermal units.

If we assume, as Mr. Buel does, that the heating value of anthracite is 17,320 thermal units, then the best result obtained at the Centennial would show an efficiency of only 67.5 instead of 80 per cent. It would be difficult to figure up a loss of efficiency in the best of these tests of 32.5 per cent. without assuming an enormous excess of air in the flue gases, an enormous amount of radiation, or else imperfect combustion. It is not conceivable that either of these conditions could be allowed in the five tests showing the best result, and we are forced to the conclusion therefore that the heating value of anthracite coal is not anything near 17,320 thermal units per lb. of combustible, but is more nearly 14,500 thermal units.

But Mr. Buel may answer, all this is pure theory, and is opposed to the actual results obtained from the experiments of Isherwood on Marine boilers, and the experiments on the Stead boiler. The rejoinder is, that the theory is based on a larger number of facts, and better attested than Isherwood's or Stead's, since in neither of these cases was there any competition between rival boiler makers, nor is there any evidence that such precautions were taken to insure accuracy as were taken in the tests at the American Institute Fair and at the Centennial. So convinced am I of the fact that the heating value of American anthracite is not materially greater than 14,500 thermal units per lb. of combustible, and that it is not possible to obtain more than a trifle over 12 lbs. of water evaporated from and at 212° per lb. of anthracite combustible in any high pressure boiler with natural draft, that if I should obtain a higher result in a test, I would discredit

my own figures, and make the test over again.

In column II. I have introduced some figures of tests of Babcock & Wilcox boilers in addition to those given by Mr. Buel. No. 10 is from a test by Mr. Geo. H. Barrus, using a mixture of anthracite screenings and Powelton semi-bituminous. It shows two facts, 1st, that the semi-bituminous coal has a greater heating power than anthracite; 2d, that although the boiler was driven at a very high rate of evaporation it still showed excellent economy, and indicates an error in Mr. Buel's statements that water-tube boilers are not economical at high rates of combustion.

Nos. 11 and 12 are two tests of the same boiler made by myself. No. 11 was with poor pea coal, and No. 12 with clean egg. The higher rate of combustion shows the best result. No. 13 was a test made by my assistant with a semi-bituminous coal. It confirms Mr. Barrus' test, No. 10, in showing that the semi-bituminous coals have greater heating value per lb. of combustible than the anthracites.

Mr. Buel makes another thrust at sectional boilers in respect to their safety. Instead of taking an English experience as old as 1878, in which 79 per cent. of all the boilers are of patterns rarely used in this country, and only 15 per cent. are externally fired boilers, he might have taken present experience in this country, with probably 79 per cent. of externally fired boilers, which are continually exploding with great loss of life, and probably 15 per cent. of sectional boilers, none of the chiefly used and properly designed varieties of which have ever yet been known to explode, except in detail, doing no damage to life.

Considerable space is taken by Mr. Buel in proving that in a boiler with good furnace heating surface this surface evaporates the greater part of all the water that is made into steam by the boiler. In any boiler in which the furnace heating surface bears a large ratio to total heating surface this must follow as a necessary consequence of the law that the transmission of heat varies approximately as the square of the difference of temperatures on the two sides of the plate. It does not nevertheless follow that it is best to design a boiler with large ratio of furnace to total heating surface. The writer has made experiments with Babcock & Wilcox boilers in which the furnace heating surface was materially reduced, by protecting it from radiation from the fire by means of a fire-brick arch. The results were as well as could be expected with the coal used (Penn'a. & Illinois bituminous), and very much better than he obtained with the same coal and the same boiler without the arch, or with the same coal and different boilers, in which the ratio of furnace to total heating surface was quite large.

Mr. Buel depends for a considerable portion of his argument upon the theoretical efficiencies derived from Rankine's formula. To show how much dependence should be placed upon this formula it may be well to refer to Rankine, (Steam Engine and Prime Movers p. 293).

"Hence it may be *expected* that the efficiency of a furnace will be expressed to an *approximate* degree of accuracy by the following formula $\frac{E}{E'}=\frac{SB}{S+AF}$ in which A is a constant which *is to be found empirically*, and is *probably* proportional *approximately* to the square of the quantity of air supplied per lb. of fuel." "B is a fractional multiplier to allow for miscellaneous losses of heat, *whose value is to be found by experiment.*" "The formula is of course not intended to supersede experiments and practical trials, nor to give results as accurately and satisfactorily as such experiments and trials, but to furnish a convenient means of *estimating approximately* the evaporative power of fuel in proposed boilers, and the comparative efficiency of different boilers. The formula is framed on the supposition, etc." Rankine's formula was first published in 1859.

In so far as his constants are based on experiment, they are on experiments with boilers of a class not often used in this country for stationary purposes, and with coals which are in every way dissimilar from our anthracites. It is useless therefore to predicate anything concerning the various types of boilers not

known to Rankine in 1859, and fired with coal of a character not known to him, by means of his formula, based as it is on probabilities and suppositions, without first correcting the constants in it, as Rankine himself directed, by means of experiment.

Lest the length to which this article has grown may lead to some confusion in the mind of the reader as to the exact points of my disagreement with Mr. Buel I will restate briefly my positions as follows:

1st, There is no reason to believe that American anthracite coal has a higher heating value than 14,544 thermal units per lb. of combustible.

2d, It is probable that the average air supply to furnaces burning anthracite coal, when it is thoroughly burned, with natural draft, is not much less than 24 lbs. per pound of combustible.

3d, That any test of a boiler at high pressure, with American anthracite coal, and natural draft, which shows a result exceeding 12 pounds water per pound of combustible should be discredited, at least until competing tests by a board of experts, such as the American Institute Fair or the Centennial tests, shall have shown that a higher result can be reached in practice.

4th, That while heating surface in the furnace may transmit more heat to the water than surface which is beyond the furnace, it by no means follows that greater economy will be obtained by making the ratio of furnace to total heating surface greater than now prevails.

5th, That no sound argument can be based on Rankine's formula in relation to American anthracite practice, until the constants in it are corrected by experiments for the particular class of boiler and of fuel in question.

6th, That water-tube boilers do not give inferior results at high rates of combustion, if they are properly proportioned for such high rates, to any greater extent than any other class of boiler.

7th, That notwithstanding the letter of an English authority in 1878 which shows that a certain Insurance Co. had no explosions, disastrous explosions are very common in this country with ordinary shell boilers, while no serious explosion has been known of a boiler properly called sectional.

8th, That the test of the Stead boiler, No. 1, Table XXII., must be discredited, and it is not probable that the Stead boiler can be made to show any better results than 12 pounds of water per pound of combustible when run under high pressure and natural draft, and with coal similar to that used in the Centennial tests.

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THE TENSITY AND PRESSURE OF DETONATING GAS MIXTURES.—MM. Berthelot and Vielle have recently been studying the influence of the density of detonating gaseous mixtures upon the pressure developed. The measure of pressure developed by the same gaseous system, taken under two initial states of different density to which the same quantity of heat is communicated, is an important matter in thermodynamics. If the pressures vary in the same ratio as the densities, we may conclude, independently of all special hypothesis on the laws of gases, first, that the specific heat of the system is independent of its density (that is to say, of its initial pressure), and depends only on the absolute temperature, whatever that may mean; and secondly, that the relative variation of the pressure at constant volume, produced by the introduction of a determinate quantity of heat, is also independent of the pressure and a function only of the temperature. Lastly, the pressure itself will vary proportionally with the absolute temperature, as defined by the theory of a perfect gas, and will serve to determine it. MM. Berthelot and Vielle operated with a bomb, at first kept at ordinary temperatures in the air, and afterwards heated in an oil bath to 153 deg. Cent. They also employed isomeric mixtures of the gases, methylic ether, cyanogen, hydrogen, acetylene, and other gases were experimented upon, and the general conclusions are as follows: 1. The same quantity of heat being furnished to a gaseous system the pressure of the system varies proportionally to the density of the system. 2. The specific heat of the gas is sensibly independent of the density as well towards very high temperatures as about 0 deg. Cent. This is all true for densities near to those that the gas possesses cold under normal pressure, and which varied in the experiment to double the original value. 3. The pressure increases with the quantity of heat furnished to the same system. 4. The apparent specific heat increases parallel with this quantity of heat. These conclusions are independent of all hypotheses on the nature and laws of gases, and were simply drawn from the experiments in question.

# EXPERIMENTS WITH COMPRESSED GUN COTTON,

CONDUCTED IN THE MANUFACTORY OF MAX WOLFF & CO., WALSRODE.

By the Superintendent, MAX VON FORSTER, Engineer, Premier-Lieutenant a. D.

Translated from the German, with consent of the Author, by Lieut. JOHN P. WISSER, U. S. Army.

The following experiments have for an object to determine in what manner compressed gun-cotton should be applied to obtain the greatest and most useful effects. Many experiments have already been made with this object; it is difficult to arrive at correct results since we have no accurate instrument for measuring the force of quick-burning explosives.

Nevertheless, the author believes that, assisted by his long-continued practice in military and civil work with explosives, he has been able, supported by the following experiments, to arrive at an approximately correct conclusion on the effects of the various kinds of explosives.

The differences in effect between the less rapid-burning and the more rapid-burning, between the rapid-burning and the slow-burning, are so marked that they may be easily observed, and compel the admission that certain conclusions may be drawn therefrom.

Moreover, as superintendent of a gun-cotton factory, the author has the opportunity, and it is in fact his duty, to work with this explosive; material, time and place are always at his disposal for these investigations, so that he may hope to have made some progress in the preparation of gun-cotton, as well as a few observations on its properties. The author will, nevertheless, be very grateful for every correction, and also for any contributions on other experiments in the same field, and will gladly modify his views through other obtained results, continuing, meanwhile, his own researches.

The gun-cotton used is, unless otherwise stated, such as is prepared in Germany and other states for military purposes, and is made in the factory of Messrs. Wolff & Co., Walsrode. It contains 12.3 per cent. nitrogen in the ash-free substance, and is in the form of cylinders 30 mm. in diameter and 70 mm. or more in height, and has a specific gravity of one; a few pieces of larger diameter, having a specific gravity of 1.1 are also employed.

By *dry gun-cotton* is understood that which has been dried until its weight is constant, and has then absorbed about 1 per cent. of moisture from the atmosphere.

*Wet gun-cotton* has absorbed in 100 parts of gun-cotton by weight the parts of water given, mostly 25 per cent. For the detonation of dry gun-cotton primers (called detonators) are used, containing 1 grm. mercury fulminate, which are fired partly by electricity, partly by means of Bickfords' quick-match.

The object to be exploded consists of a leaden cylinder 46 mm. in diameter, set upon a small iron plate lying on the solid ground. The cartridges are placed on the leaden cylinder.

It is to be remarked that the cartridges have a greater effect on the shorter than on the longer leaden cylinders, that therefore only those explosions, which occurred on cylinders of the same height, can be directly compared.

The annexed drawings exhibit the leaden cylinders in cross-section and plan, after the explosion.

Following the experiments on the effects of explosion are given certain observations on, and other experiments with, compressed gun-cotton.

## 1. EXPERIMENTS ON THE EFFECTS OF EXPLOSION.

In Plate 1 the explosions Nos. II., III. and IV. were made with cartridges of 50 grm. gun-cotton 70 mm. long; Nos. I. and V. with cartridges of greater weight, but also of greater length; the increased weight had no increased effect, on the contrary, the effect in case of the longer cartridges was diminished.

The explosions VII., VIII. and IX. were produced by cartridges of the same size as II., III. and IV., not solid, how-

ever, but hollow, as shown in Plate 1, Figure 2.

The effect on the leaden cylinder is greater in case of the hollow cartridges although the solid ones were greater in weight.

The explosions VI. and X. are analogous to I. and V., with cartridges of greater weight, but also greater length, than VII., VIII. and IX. The effect of both is again less than of those conducted with less weight.

*It follows therefore, that in case of dry gun-cotton no increased effect is produced by increasing the charge, if at the same time the length of the charge is increased; furthermore, it appears that in a long cartridge of dry gun-cotton the upper parts cannot transmit their detonation to the lower parts in the same explosive shock as that effected by the initial detonation of the primer, and hence it follows that in long charges of dry gun-cotton it is better to use several primers, placed some distance apart, and to fire them by electricity, in order to produce the greatest possible effect of the total charge.*

These relations are more apparent in case of gun-cotton mixed with saltpeter and of wet gun-cotton.

In Plate 2, IX. is a cartridge of gun-cotton intimately mixed with 50 per cent. barium nitrate ("Baryt saltpeter"), constituting an almost homogeneous mass. The cartridge is 70 mm. high, weighing 65 g.; the explosion VIII. is of a similar cartridge, but 220 mm high and weighs 225 g.; now, although the effect of the latter cartridge is not increased in proportion to the increased weight, it is nevertheless greater on the whole, and indeed considerably greater than in the former cartridge, while in the case of dry gun-cotton the effect was diminished.

Explosion X. consists of two wet cartridges with 25 per cent. water, and a dry one of 50 g. for the production of the detonation.

Explosion XI. consists of three wet and one dry cartridges.

The effect of these two explosions is the greatest of those thus far described; it is considerably greater than that of the dry priming cartridge alone, and much greater than that of the long charges of dry gun-cotton. In case of the wet gun-cotton the detonation was propagated with at least equally great suddenness throughout the entire length of the charge. Moreover, the wet gun-cotton is, on the whole, more sudden in its effects than the dry.

The dry gun-cotton depresses the center of the leaden cylinder and bends the edges over, but both parts have, nevertheless, a smooth surface; in case of the wet gun-cotton the part of the lead referred to was torn and pulverized.

Similar effects have been obtained with the very sudden explosives prepared from nitric acid and binitro benzole, as well as from nitrous acid and sulphocarbides.

Professor Abel, in his highly important experiments, described in Dingler's Polytechnisches Journal, 1874, has shown that the rapidity of propagation of the detonation of dry gun-cotton is considerably less than of wet gun-cotton; the above experiments show that this is also true with respect to the *effects* of the two kinds of gun-cotton; furthermore, that in a long charge the effect itself of the dry gun-cotton diminishes towards the end of the charge, while that of wet gun-cotton does not.

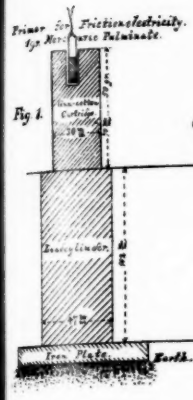
Professor Abel found, further, that in long charges of gun-cotton the rate of propagation of the detonation was uniform, being as great in the last meter as in the first. But he used gun-cotton with 30 per cent. water; for, otherwise would have been the results had dry gun-cotton been used. This will probably have a slower rate of detonation towards the end of the charge.

Professor Abel says, moreover, p. 318, Dingler's Journal, 1874:

"The more energetic action produced when gun-cotton and its preparations are detonated in a moist condition, has furnished the striking proof that the detonation is transmitted the more easily, and the transformation of solid to gas and vapor takes place the more suddenly the less the compressibility of a given explosive mass subjected to the action of a sufficient initial detonation. Since the water replaces the air contained in the compressed masses, the propagation of the detonation will evidently be favored by the increased resistance which the particles offer during the instant of detonation."

True as these propositions are they do not explain the difference in action re-

# Plate I.




I.
Cartridge 30 ℔ diam., 110 ℔ high.
80gr. wght., depressed 14 ℔.

II.
Cartridge 30 ℔ diam., 70 ℔ high.
51gr. wght., depressed 24 ℔.

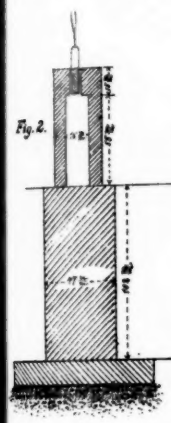
III.
Cartridge 30 ℔ diam., 70 ℔ high,
51gr. wght., depressed 13 ℔.

IV.
Cartridge 30 ℔ diam., 70 ℔ high,
30.3gr. wght., depressed 8 ℔.

V.
Cartridge 30 ℔ diam., 210 ℔ high,
113gr. wght., depressed 13 ℔.
Spandauer Primer.

Elevation.

Plan.




VI.
Cartridge 30 ℔ diam., 110 ℔ high,
60gr. wght., depressed 20 ℔.

VII.
Cartridge 30 ℔ diam., 70 ℔ high,
49gr. wght, depressed 23 ℔.

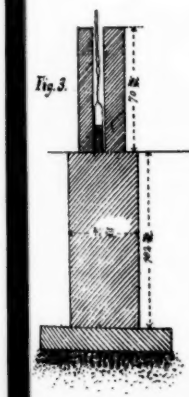
VIII.
Cartridge 30 ℔ diam., 70 ℔ high,
46gr. wght, depressed 21 ℔.

IX.
Cartridge 30 ℔ diam., 70 ℔ high,
33.7gr. wght., depressed 25 ℔.

X.
Cartridge 30 ℔ diam., 210 ℔ high.
119gr. wght., depressed 16 ℔.
Spandauer Primer.

Elevation.

Plan.




XI.
Cartridge 30 ℔ diam., 70 ℔ high,
51gr. wght., depressed 7 ℔.

XII.
Cartridge 30 ℔ diam., 70 ℔ high,
51gr. wght., depressed 4 ℔.

Elevation.

Plan.

1:2.

ferred to between dry gun-cotton and wet gun-cotton containing 30 per cent. of water. The latter is more easily compressible and less compact than dry, and is much more easily subject to being crumbled and torn up than the dry. When it is desired to work over dry pieces of gun-cotton in the factory, which must therefore be first crumbled, they are moistened, because they are then more easily crumbled. The particles of a wet cartridge will probably offer a much smaller resistance to the initial detonation against tearing and dispersion than those of a dry cartridge.

The initial detonation acts principally by the shock; when this strikes a dry cartridge filled with air the air acts to diminish the shock: because it is elastic it is compressed and transmits a slighter shock than it received. The water in a wet cartridge is not compressible; it transmits the received shock with a force equal to that which it received.

The explosions Plate 2, I., II. and III., exhibit a clear view of the necessity of giving the charge and the object to be destroyed the largest possible surface of contact, and the charge itself the least possible height.

No. I. is 18 mm. high, No. II. only 13 mm., but has a larger surface of contact with the leaden cylinder; the latter has produced a greater effect than I. in spite of its less weight; explosion III., with triple the weight, but also triple the weight of charge of II., has accomplished but little more than the latter.

The same effect of the form of the charge is shown in Plate 2, explosions IV. and V., and also VI. and VII.

Explosion VI. has a flat charge, lying close to the object, and has acted with considerably more force than VII., in which the same weight of gun-cotton is applied in a high and narrow form.

In all these results it is remarkable that the difference of effect is already apparent with small differences in dimension; a charge 13 mm. high acts more energetically than one 18 mm. high, and a still greater difference is found in the action of one 30 mm. high, and another 68 mm. high of equal weight.

If we place a piece of compressed gun-cotton on a piece of iron, an accurate impression of the form of the under surface of the gun-cotton is produced by the detonation; the gases acting on the iron have occupied exactly the same space, and no more, that the solid explosive previously occupied. As long as the detonation gases act they have not yet expanded on the whole.

Every angle, every depression, every indentation present in the piece of gun-cotton, is copied by the gases and impressed on the underlying iron, and we may also conclude herefrom that only the gases evolved by the very undermost layer of gun-cotton act on the iron, while the others are lost.

If we observe the action of compressed gun-cotton, as compared with that of dynamite, on compact rock, with the application of drill holes, it will be found that gun-cotton destroys the interior of the drill hole, that it completely pulverizes the rock, but that it does not eject the pieces as dynamite does. The action of gun-cotton seems to be more sudden, but more local, than that of dynamite.

If we could give the gases of the detonated gun-cotton a fixed direction towards the object the effect would be increased. The experiments here given furnish two means of attaining this end to a certain degree.

In Plate 1 the primers are set in the cartridge on the side opposite the object to be destroyed in all the explosions, except XI. and XII., in which the primer is placed in that part of the cartridge which is in contact with the object. The effect of these last two explosions is much less than that of the others.

The detonation of the primer gives the detonation gases a direction away from itself, a direction towards the side opposite.

This fact may be explained by the transfer of the vibrations of the gases of the primer to those of the gun-cotton, when the gases of the primer have a fixed direction. Simpler is the explanation that the detonation gases of the primer form, to a certain extent, a dam, so that they offer from the beginning a resistance to the gases generated from the gun-cotton, and thus press them toward the other side. Even a very weak dam of loose earth acts on the gases of the explosive, why not the energetic detonation of the mercuric fulminate?

At what point in the cartridge the original direction of the gases is main-

tained has not yet been determined; according to the experiments here given, the distance does not seem to be very great, but the direction given by the initial detonation, in case of a cartridge of a weight of over double the diameter, 70:30, becomes quite noticeable. In explosions in a closed space the same phenomena occur, as illustrated in Plate 2, explosion XV.

The cartridge is here enclosed in a block of lead, and the opening, through which the cartridge was introduced, is closed by a leaden stopper. The latter is fastened with iron bands. The cartridge is therefore enclosed equally firmly on all sides. The primer is not introduced into the cartridge but is simply in contact with it.

In this explosion, a circumstance unfavorable for this phenomenon was that the enclosing of the cartridge was not complete, since the iron bands, which held the leaden stopper, were fractured by the explosion, so that the stopper was carried away, and gases were enabled to escape prematurely on the side of the primer. Otherwise the action would have been even more energetic than it actually was.

*The primer must therefore always be placed on the side opposite the charge.*

The second method of giving the detonation gases a fixed direction, consists, according to the experiments, in furnishing the cartridge with a hollow opening, on the side opposite the primer and adjacent to the object to be destroyed. Explosions VI.-X., Plate 1, furnished with hollow cartridges are, on an average, considerably more violent than I.-V., although the latter were of the same size, but greater in weight, since they were solid.

An increased action, in case of the hollow cartridges, takes place exactly in the direction of, and over the entire cross-section of the opening. More clearly is this shown when, in place of lead, the more brittle metal, iron, is taken as the object to be destroyed.

In explosions XII.-XIV., Plate 2, wrought iron was selected as the object. In XIII. and XIV. a strong impression of the cross-section of the hollow opening of the cartridge appears, which is twice the depth of the impression produced by a solid cartridge as in No. XII.

*On the whole, the effect of hollow cartridges is greater than of solid cartridges of equal size though greater in weight.*

*In explosions where the space, in which the explosive charge is placed, is limited, as in the explosive charges of shells, in which the explosive charge and the object to be exploded cannot receive a larger surface of contact than that of the interior of the shell, it is best to apply both means described, viz.: the hollowing of the charge and the placing of the primer at the base of the shell, in order to obtain an increased action towards the head of the shell, in which direction also the object to be destroyed will almost always lie.*

*The use of wet gun-cotton instead of dry will also offer extraordinary advantages.*

In the application of gun-cotton it must be added that, aside from the fact that the action of the explosive charge is greater, the wet gun-cotton, as is well known, is much less dangerous in the preservation and handling thereof than the certainly very slightly dangerous dry. The combustion of a large supply of gun-cotton, and hence a loss thereof by fire, will be prevented by wet storage of it.

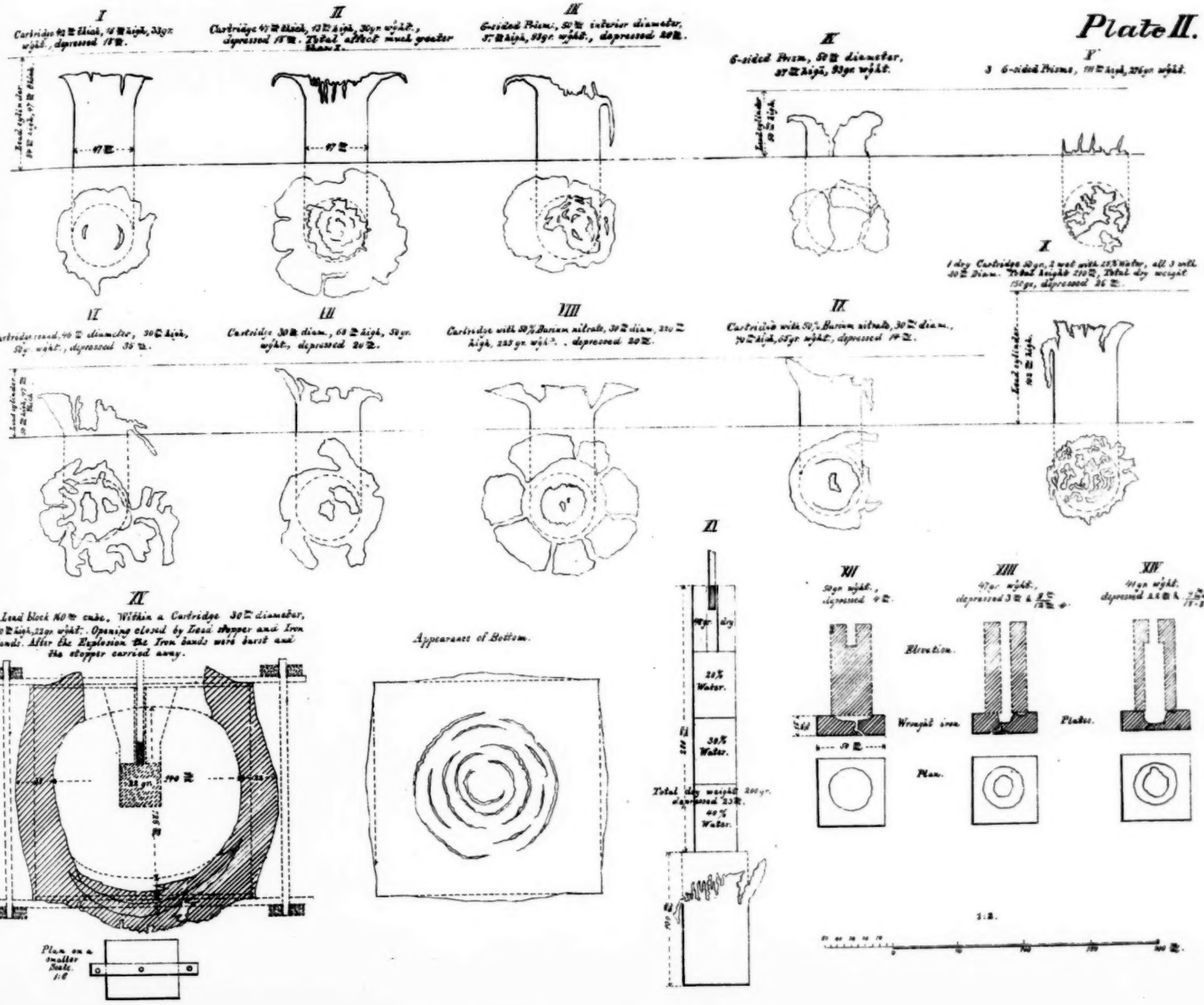
### 2. COATING OF GUN-COTTON BY MEANS OF A SOLVENT THEREOF.

Compressed wet gun-cotton has many inherent disadvantages: the water evaporates readily, and the cotton must therefore be moistened again from time to time (although this may be greatly diminished by good packing and storing in magazines), moreover wet gun-cotton crumbles easily, and even very well packed wet gun-cotton may suffer much in long-continued transportation, finally it is subject to the formation of fungus, which will in time destroy its structure. It may be remarked, in passing, that the formation of fungus is favored by the presence of paper in the packages, which shall therefore be carefully avoided.

The author has, however, discovered a process for avoiding or largely diminishing the above-mentioned objections to wet gun-cotton. It is as follows:

If we dip pieces of wet gun-cotton for a short time, $\frac{1}{4}$–$\frac{1}{8}$ minute, into good acetic ether, into nitro-benzole, or into other solvents of gun-cotton, then take them out and allow the solvent to evaporate,

# Plate II.


I
II
III
IV
V
VI
VII
VIII
IX
X
XI
XII
XIII
XIV
Appearance of Bottom.
Elevation.
Plan.
Wrought iron.
Plaster.
Lead cylinder
Water.
Total dry weight
Plan on a smaller Scale.

there will be formed on the piece of gun-cotton a very thin, but very compact coating, which gives it a very firm shape, protects it against crumbling and prevents fungoid growth.

This coating, which consists of dissolved gun-cotton, and which should be absolutely water-tight, nevertheless does not protect completely against the gradual drying of wet gun-cotton; there are formed in the coating very fine, invisible cracks, through which the water, though slowly, evaporates.

A piece of gun-cotton not thus coated, containing 25 per cent. water, will become air-dry (*i.e.*, with 2 per cent. moisture) in summer in a current of air, in several days, whereas in case of gun-cotton coated by means of ether, etc., several weeks will be required. The dried piece placed in water takes it up again. But this does not proceed very rapidly; in case we wish to effect the absorption of moisture quickly, in 1–1½ hours, we can, in the process of manufacture, leave little uncoated spaces in the coating, through which the water may penetrate rapidly.

The process described can also be applied with advantage to dry gun-cotton.

If we coat a piece of dry gun-cotton, supplied with an opening for the reception of the primer, by means of a solvent, the gun-cotton will *first*, as in the case of wet gun-cotton, receive a firm, permanent form, and *secondly*, the cartridge, as well as the opening for the primer, will be protected against moisture, without interfering with the introduction of the primer, as would be the case were the opening specially closed by a sheet of paper, and also without diminishing the capacity to explode, as would be the case were the opening lined with copper, etc. The thin coating of gun-cotton offers no resistance to the action of the primer; many experiments have proven that such cartridges will detonate with a ½-grm. primer. Moreover, such a cartridge can be used under water directly, without the necessity of introducing the primer water-tight.

It is possible to prepare pieces of gun-cotton, by means of such a coating, so water-tight that when placed in water for several weeks they will not increase in weight; however, this does not always succeed as in this case also small cracks seem to be formed. The author has therefore more recently coated only the walls of the opening for the reception of the primer by means of ether, and rendered the other portions impermeable by dipping them in melted paraffin. By this means water-tight cartridges have been uniformly obtained.

The opening for the primer may also be made water-tight, by lining it with copper, etc., but there follows a great objection in that the capacity to explode readily suffers, since, *first*, a layer of air forms between the gun-cotton and the lining, *secondly*, the lining must be broken by the primer, and *thirdly*, another layer of air, or, in case of explosion under water, a layer of water, exists between primer and lining.

It happened often to the author that, in explosions under water, when the priming cartridge was adjusted by a copper lining, in spite of the use of the best primers, the gun-cotton charge began to burn indeed, or to decompose into dense red fumes, nitrous acid fumes, etc., but did not detonate.

This decomposition of gun-cotton into nitrous acid fumes, etc., caused by an insufficiently energetic initial detonation of the primer, appears not to have been elsewhere noticed.

In the manufacture of gun-cotton, during the nitration of the cotton, this decomposition is of common occurrence when the temperature rises too high.

The process of rendering the opening for the primer impermeable through ether coating may furthermore be of importance when wet gun-cotton must remain in close contact for a considerable time with the dry priming-cartridge, since, in case it is not made impermeable, it is to be feared that the evaporation from the wet gun-cotton will moisten the dry. This takes place very easily and rapidly, and can hardly be avoided in case one may wish to use both wet and dry gun cotton as in shells.

The principal advantage of the process described is that it gives the wet gun-cotton a permanent form, and thus renders it applicable in war.

### 3. SPONTANEOUS DECOMPOSITION OF GUN-COTTON.

The author is in possession of a piece of compressed gun-cotton of the year 1878, which, in the process of manufac-

ture, was not completely washed, so that some acid remained. The piece was preserved during this time in a chest kept in a dry place. It is now completely decomposed, and has become a greenish, soft mass, which has lost the structure and appearance of gun-cotton, and, when pressed, yields a glutinous liquid. It smells sour, burns when ignited with a white flame (gun-cotton with a red flame) and emits no appreciable vapors. In an air-tight preserving-jar, in which a part of this experimental prism of gun-cotton was preserved, no pressure has been noticeable since the decomposition has set in.

This phenomenon shows that the spontaneous decomposition of poor gun-cotton takes place without the production of flame, and the author believes that a spontaneous combustion of gun-cotton has never occurred. Of course, gun-cotton subjected to high temperatures, such as can only be brought about artificially, temperatures above 120° C., is not here included. Good gun-cotton is sufficiently stable for all practical purposes.

How many unforeseen accidents may cause combustion is proven by the following experiment, communicated to the author by Professor Kraut, in Hanover.

If we take a good handful of simple cotton wadding, set a portion of it on fire and wrap the other part around the kindled point, so that the air is excluded, the piece of wadding may be wrapped in paper and preserved for months.

If, at the end of this time, the wadding is unwrapped and air admitted, the part kindled months before goes on burning and consumes also the rest of the cotton.

How often may this phenomenon have occurred when, in case of a combustion of ordinary cotton in large bulk, it was said to be due to spontaneous combustion. With gun-cotton it was probably often the same.

### 4. AN EXPLOSION UNDER WATER.

It would be of great interest if a charge of gun-cotton, which has been placed under water, could be preserved explosive for a fixed time, and be so arranged that, after another fixed time, it will cease to be explosive, and thus render itself inexplosive.

This would be of importance in all blastings of rocks in the beds of rivers, in harbors, etc., because in these cases ships and workmen in diving-bells and diving-clothes must approach the point of explosion in order to undertake the work of clearing away, and to accomplish this work pounding and striking with heavy iron tools is necessary, by which after-explosions of charges which had missed fire may occur.

It may furthermore be of importance in the laying of sub-marine mines, which need be operative but for a limited time, and must not be dangerous to our own shipping thereafter, and must moreover be capable of being raised without danger.

The author hereof has discovered a method of procedure which meets these requirements.

When a cylinder of compressed gun-cotton is so enveloped that only the bottom, that is, the part opposite the primer, is not water-tight, and is set in water, the latter will gradually penetrate into the cartridge from the bottom upward and thus wet it. When the water has nearly reached the primer, and an amount of dry gun-cotton is therefore present, not sufficient, even if it detonates, to cause the detonation also of the wet gun-cotton, the charge or mine is no longer capable of detonating, and becomes perfectly safe.

In a short while the rest of the dry gun-cotton becomes wet and the priming will be entirely useless.

Therefore, in explosions with gun-cotton under water a priming-cartridge, as above described, is used, the rest of the charge being composed of wet gun-cotton.

The priming-cartridge is easily enclosed so that only the bottom thereof is permeable to water by enveloping it in a rubber tube open below, in the upper part of which the priming is tied water-tight. The cartridge may also be made impermeable by means of paraffin or a coating of ether, and the bottom then cut off. The coating with ether has the great advantage that the opening for the primer is made impermeable at the same time. The coating produced by ether is sufficient also in itself, without the rubber tube.

In the case of sub-marine mines many other absolutely safe means naturally offer themselves for the preparation of

the priming-cartridge, according to the same system and for the purpose in view.

A cartridge of pure gun-cotton, of specific gravity 1.1, 100 mm. high, remains explosive for about 8 hours, and in 11 hours is completely wet and entirely incapable of being exploded by shock. A cartridge of gun-cotton mixed with saltpeter remains dry longer than one of pure gun-cotton.

The specific gravity, the length of the cartridge, and the extent of surface through which the water can penetrate, constitute factors sufficient to determine satisfactorily, within certain limits, the time during which the charge will remain explosive, since, with proper care, cartridges may be furnished sufficiently uniform in character to produce a uniform action.

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# ELECTRICAL CONDUCTORS.

By WILLIAM HENRY PREECE, F.R.S., M. Inst. C.E.

Proceedings of the Institution of Civil Engineers.

## II.

DISCUSSION.

Mr. Brunlees, President, said he thought the discussion might well be taken in three parts. First, as to the qualities which electrical conductors should possess; next, as to the manufacture of the conductors, and then, having the electrical conductor according to the conditions laid down by men of science, and according to the possibilities of manufacture, on the practical use of the conductor. He would first ask Mr. Preece if he desired to add anything before the discussion commenced.

Mr. Preece expressed his deep regret that one seat at the Council table was empty, a seat that had been filled by one who had taken a prominent part in the discussion of every electrical paper brought before the institution. The late Sir William Siemens had taken the deepest interest in the subject, and had succeeded very largely in enriching it. Mr. Preece had brought forward many facts to illustrate the improvement effected in electrical conductors during the last quarter of a century, and he hoped that the discussion would elicit other facts, showing how the mechanical and electrical qualities of the various metals had been almost metamorphosed. The chief point that he desired to emphasize was the position that copper was likely to take in the future, and the remarkable mechanical powers that it evinced. He had brought for inspection a number of specimens illustrating the behavior of metals and showing how they were produced. In speaking of them in his paper he had been compelled frequently to use a term which was familiar in the mouth of every electrician as a household word, but which had not yet become familiar to engineers—the term "resistance." He had spoken throughout the paper of the resistance of wires, knowing well, as an electrician, what the term resistance meant; but many engineers assumed that it meant mechanical resistance—resistance to tension or to rupture. It was a misfortune that the term had been too freely introduced, and it would have been better if he had spoken entirely in terms of conductivity. If he had said that a copper wire conducted electricity six times better than iron wire of the same dimensions, he should have been better understood than by saying that a piece of wire had one-sixth of the resistance, or six times the resistance, as the case might be. Sir William Thomson had pointed out in that room how it was that conductivity was the exact reverse or reciprocal of resistance, and he went so far as to suggest that there ought to be a standard for conductivity, as there was a standard for resistance. That point had not, however, yet been reached. But, should he still use the term "resistance," he wished it to be clearly understood that he meant by it the reciprocal of conductivity. Resistance was expressed in terms of a standard called the ohm, and he believed the day was not far

distant when every one would know what an ohm was as well as what a foot was, or what a meter was. The ohm standard was expressed by a certain length, a column of mercury, about 1 meter in length, of 1 square millimeter section. But in dealing with copper and with iron, a standard had been introduced expressed in weight per mile. Copper wire weighing 842 lbs. per mile gave an ohm, and was called a mile ohm of copper; iron wire weighing 4,500 lbs. gave a mile ohm of iron; so that the relative dimensions of that curious standard depended both upon length and weight. He had brought for exhibition the various instruments that were used to test the metals, and he would show the test upon iron, and, if time permitted, upon copper, for he wished the members to have distinct evidence of the remarkable mechanical qualities of the copper produced at the present day. The copper wire specimens on the table possessed a breaking strain of 30 tons on the square inch, while the iron used for telegraphic purposes possessed a breaking strain of only 22 tons. Again, there was an impression that copper was very plastic, and that it stretched very considerably; but he had figures showing that it did not stretch so much as iron; indeed, the copper wire then used stretched considerably less than iron; so that in both properties, tensile strength and elasticity, the copper of the present day was better than the iron. The machine exhibited was that used for testing the breaking strain. There was a system of levers and a shifting weight. As at present arranged, the machine would register from 1 lb. to 5,000 lbs. There was a length of wire applied which was clamped in such a way that there was no nipping or biting, and it suffered no mechanical injury. When, by means of the wheel and the multiplying gear, a slow motion was brought to bear, a weight was gradually applied to the wire until it broke. The piece of wire tested weighed 400 lbs. to the mile, and it was known as 7½ by the centimeter gauge. (The wire was then tested for tensile strength.) The wire, it would be seen, had broken at 1,160 lbs., the specified breaking strain being 1,080. It was also necessary to record the stretch, which was sometimes done at the same time; but it was better to do it independently. The second machine exhibited, clamped, a length of ten inches. (The wire was here tested for stretch.) The wire, it would be seen, had given way after stretching 15 to 16 per cent. Another test was the torsion test—a test for torsive capacity. It was not desired that the wire should resist torsion, but should bear torsion. By making an ink mark along the top, and by twisting the wire in the other machine until it broke, a series of spirals was formed, and the number of the spirals indicated the torsive capacity. All wires were specified to show a certain number of twists. The wire experimented upon was taken from a coil of No. 7½, and all the pieces had been cut off the same length. He would next experiment upon copper wire No. 14. Meanwhile he would refer to the enormous advantage which copper wire would give for overhead purposes. There was then going on what he had characterized as a meaningless crusade against overhead wires. Vestries and other small corporations in outlying districts were endeavoring to force the Government to put the wires underground, and the newspapers were bringing forward the fact that even in Germany and France the wires were so placed. It was not sufficiently known that Germany and France were only doing what had been done in England a quarter of a century ago. The wires were then placed underground; all the main wires in the country between London, Birmingham, Liverpool, Manchester, Leeds, and all the wires to the cables on the south coast, were underground; but after sufficient experience of their use it was found necessary to take them up again; and he should not be in the least degree surprised to find the same result taking place, sooner or later, in Germany and France. It had been found that wires overhead had many times the efficiency of wires underground, and it was absurd to try and force electricians to do an act that was commercially wrong and practically foolish, for the imaginary reason that overhead wires were supposed to be dangerous. Statistics showed that in the United Kingdom there were 18,000 persons killed by accident every year. The average for the last thirty years—during the period of telegraphs—was about 12,000

per annum. In that period about 400,000 persons had been killed by accident, and of that number two only had been killed by telegraph wires. One of those accidents was owing to the foolish practice he had lately seen suggested, even in the *Times*, of having the wire tied to the railing of an area. If, in that case, the wire had been allowed to hang free, it could not have produced any injury. With the light, fine copper wires at present used, it was scarcely possible to conceive that if they did break they could produce any harm. Why, then, should legislation be enacted for such a minimum of accident as was shown by the statistics to which he had referred? He granted that the wires were very ugly, and no one going through Moorgate Street or Leadenhall Street could help being vexed to see the metropolis so much disfigured. That, however, was not because the wires were overhead, but simply because they were carried in a higgledy-piggledy, spider-web fashion, in any direction, without method or system. If the corporations wanted to do good it should not be by trying to force the authorities to place the wires needlessly underground, but by trying to exercise some control over those who erected wires overground. It had been thought, from the remarks he had made about the gauge, that he expressed disapproval of the gauge recently introduced by the Board of Trade; that, however, was not his object. The gauge was a very good one. It had been accepted by nearly everybody; it had been authorized, and it would do a certain amount of good. But what the Post Office objected to was that there should be so many changes. When it was found that a wire which had been known by their men as No. 8, was called at another time No. 7½, and at another No. 7¾, they decided, in order to avoid confusion, to abandon the gauge altogether, and simply to speak of the wire as of so much weight per mile. That method had been introduced into India with great success. He had stated that the great improvements in the manufacture of iron were due to the care bestowed upon the specifications of the authorities at the Post Office. He ought to add that the improvement was also due to the great care taken by the India authorities in improving the specification. (The copper wire was here tested.) It would be seen that the piece of wire experimented on had stretched 0.15 in 10.2 inches, or nearly 2 per cent. The other piece of wire was eighty-one thousandths of an inch in diameter, and it ought, by specification, to break at 320 lbs., which would be 27 tons on the square inch. It actually broke at 362 lbs., or 31 tons on a square inch.

Sir Frederick Abel, C. B., Honorary Member, said that his remarks should be mainly with the view of eliciting further information, and, if possible, of adding a few supplementary facts with reference to certain points dealt with in the paper. Reference had been made to the remarkably useful work done by the late Dr. Matthiessen, as far back as 1860, in regard to the influence exerted by different commercial impurities in copper upon its conductivity or electrical resistance. The author had correctly stated that those researches had not only greatly instructed them with reference to their knowledge of copper as an electric conductor, but had also led, in a very important degree, to improvements in the treatment of copper, with special reference to the production of electrical conductors. It was stated in the paper that Dr. Matthiessen had determined the electrical resistance of pure copper; but he was not quite sure whether, in the present day—twenty-four years after the experiments—whatever reliance might be placed on the results obtained, the measurements given could be accepted as truly representing what the resistance of perfectly pure copper should be. Electricians had acquired a considerable amount of additional information, and they were on the way to acquire still more in regard to the influence of very minute proportions of impurities upon the electrical conductive powers and other physical properties of metals; and no one could teach them more in that direction than Professor Hughes, who had recently been pursuing some very remarkable researches. Therefore, although they still accepted with great confidence the results of Dr. Matthiessen, they might probably learn yet more with regard to the conductivity of really pure copper. In reference to the manufacture of copper wire, the author had mentioned that soft copper always gave a higher conductivity than hard-

drawn copper, its tensile strength being, of course, considerably lower. On that point he would ask the author to furnish additional data. In some experiments that he had made, which, though few, had been carefully conducted, he had found that the conductivity of a particular sample of copper varied but little, whether it is in the hard-drawn or in the annealed state. Dr. Matthiessen had mentioned that it varied about 2 per cent., but in some recent experiments Sir F. Abel had found that there was no practical difference between the conductivity of certain samples of copper, containing but a small proportion of chemical impurity, when this was in the hard-drawn or the annealed condition. In one experiment, with a length of 15 feet 6 inches of copper, he had obtained 0.0257 ohm resistance with the hard-drawn, and 0.02515 with the annealed. The very slight variations might almost be variations due to experimental error, even though when multiplied upon a statute mile the difference was more appreciable. Taking the numbers upon a mile of wire they would represent, in the case of the hard-drawn wire, 8.707 ohms, and in the case of the annealed wire 8.520 ohms, showing even thus only a very small difference. He might be allowed to allude in passing to what the author had said in reference to what appeared to be the influence of currents upon the durability of a conductor. Electricians had still much to learn in that direction. Many persons believed that important alterations in the physical structure of copper and of metals generally, were effected by the continuous or intermittent passage of currents through them. He thought, with the author, that further information was necessary on that point. One thing was worthy of notice, namely, the influence of small quantities of impurities which would be detrimental to copper as a conductor, when contained in the india-rubber or other materials used as dielectric coatings. Minute quantities of sulphur, for example, in india-rubber, would find their way into copper wire coated with it, and exert a detrimental effect on the metal as a conductor. It was that which led Mr. Hooper to devise the very pretty method—which, however, was only partially successful—of keeping vulcanized india-rubber from direct contact with copper wire coated with it, by introducing india-rubber between the vulcanized rubber and the copper itself. As to the question of purity of copper, the author had referred to the very great advantage which had been conferred by improvements in the metallurgical treatment of copper upon the product, so far as electric conductivity was concerned. He had mentioned the "best selected" copper as the material which gave the best forms for wire which could be obtained until certain high qualities of copper were brought from Australia, Lake Superior, and other places. The quality of "best selected" copper, however, was very uncertain, because it was obtained by a somewhat rule of the thumb method, and it frequently contained proportions of impurities, detrimental to the conductivity of copper, fully as great as those which had been introduced in the production of some more recent wires, such as phosphor-bronze. One of the most important impurities in "best selected copper" was arsenic, and arsenic and phosphorus were about equal in their detrimental effect on the conductivity of copper. He should like some additional information with reference to the statement that electro-deposited copper had not the same mechanical strength as ordinary refined copper. He hardly thought that the author meant to refer simply to copper in the condition in which it was deposited. Electricians would not attempt to make wires by depositing copper in that form, and he did not see why copper obtained by electrolysis should not be as strong as copper obtained by other methods, because by re-fusing copper in a reducing atmosphere, and then converting it into bar and rolling and drawing it in the ordinary way, it was subject to the same mechanical treatment, and was obtained in the same physical condition, so that, whether it had been originally reduced by electrolysis, or by chemical means, there could be no difference, as far as he could see, in the strength of a wire of a given purity.

With regard to galvanized wires, the author had pointed out that the durability of iron wire was maintained by galvanizing; and having described the method of treatment, he afterwards stated that the zinc with which the wire

was coated became oxidized when exposed to the air, and that, since zinc oxide was insoluble in water, the wire was protected by an impervious coat. There, as a chemist, he must join issue with the author, though the point was not a very important one. The truth was that oxide of zinc was by no means insoluble, but was actually soluble in soft water, like rain water, to such an extent, that galvanized iron could not be used with advantage as a material for the construction of tanks or conduit pipes where soft water existed, on account of zinc passing into solution in so considerable a proportion as to exercise an injurious medicinal effect upon the constitution. He thought that when galvanized iron wire was exposed to the air it became coated with a hard film of oxide, such as was formed when a sheet of lead, which was also an oxidizable material, was exposed to atmospheric influences. That hard film protected the metallic coating, and prevented further oxidation; so that the zinc itself was protected by the superficial coat of oxide, which was only very slowly removed by water, the zinc itself remaining as a protective to the metal until the atmosphere plus water found out some minute imperfection in the coating, and then, as soon as the smallest portion of the iron surface was exposed, the action established between the zinc and the iron led to rapid corrosion. That was why, even in the absence of the acid vapors to which the author referred as destructive to the metal in towns, galvanized iron, not only in the form of wires but applied to other purposes, was uncertain in regard to stability; and there could be no doubt that galvanized iron, though it had been very useful, was on that account a comparatively unreliable material.

He would next refer to an interesting form of conductor, of which those who had to construct military telegraph lines on active service had had considerable experience, namely, the so-called compound wire produced by coating steel wire with a copper sheath, which was soldered to the surface by drawing the steel wire through a bath of tin which acted as a cementing or soldering material. In that way, as the author had pointed out, a comparatively light and very strong wire of not very high resistance was produced, which presented at first sight important advantages as a conductor; so important indeed did they appear to be that the Royal Engineer authorities, finding the material when first tested to present such great advantages over ordinary iron, and even over copper wires in many respects, introduced it into the service in some recent campaigns. It was used in the Ashantee and Abyssinian wars, and afterwards in South Africa, and some was even sent to Egypt, though he believed it had been returned thence in the same condition in which it was sent out. The objections advanced against it even as recently as 1881, related simply to its being rather springy, and somewhat difficult to bend, but on the other hand it was found free from the liability to kink, so that the one defect was counterbalanced by the other advantage. But after the material had been in store a few years, and issued again for a special trial at Aldershot, it was found that when attempts were made to put up a line upon which some strain was brought to bear, the wire broke in many points; indeed it broke down utterly, and this was due to the effect pointed out by the author, namely, that there existed here and there minute imperfections in the building up of the wire, which, admitting the access of water and air, had established a corrosion of the steel wire and brought about the destruction of the material which at first sight presented such important advantages. There was one other disadvantage, namely, that when the wire had been roughly used there was a tendency of the outer sheath to peel off and injure the hands of operators handling it, and to be objectionable in many ways. It was for those reasons that the wire, which had been somewhat extensively used in the service, had been discarded.

The last class of wires to which Mr. Preece had referred were the bronze wires, the first of which was the so-called phosphor-bronze. Anyone first applying phosphor-bronze, correctly so called, to the production of a good conductor would be considered very short-sighted, for phosphorus was one of the elementary bodies about the most detrimental to the conductivity of copper that could possibly be used, ranking equal with arsenic in this respect. In some of the early

phosphor-bronze wires which he examined, having a resistance of about 49 or 50 ohms to the mile, there existed 0.22 and 0.18 per cent. of phosphorus. But, as the term "bronze" implied, there was always in those wires a certain proportion, but only a very small proportion, of tin. After referring to these new kinds of wires as alloys and as bronzes, the author stated that phosphor and silicium-bronze were so called, not so much because those materials were mixed with the copper, but because they were used in the preparation of the alloy, and there was no doubt that to a considerable extent that was the case. Phosphorus was undoubtedly a powerful deoxidizing agent; and as oxygen was one of the important enemies to the conductivity of copper, the introduction of phosphorus by removing the oxygen would increase the conductivity of the metal or of its alloy with tin. But it was almost impossible to produce a wire from a metal treated with phosphorus, without leaving some traces of phosphorus in the material; hence one could quite understand how so-called phosphor-bronze, though superior in point of strength to copper itself, was inferior in regard to conductivity, and was found to be very variable, because it would be exceedingly difficult to produce phosphorized bronze or phosphorized copper at different times containing precisely or approximately the same proportion of phosphorus. The latest form of bronze wire, and the one to which the author had directed special attention, was the silicium-bronze, to which Sir. F. Abel's attention had also been directed when visiting the electric exhibition at Vienna. He there saw the very thin wires now used for telephonic purposes. He had made some experiments with two samples of silicium-bronze wire brought from Vienna, and the results confirmed the statement of the author that the term silicium-bronze did not at all correctly represent the composition of the material; in point of fact it was not to be assumed that it need contain any silicium at all. In one wire which he examined the amount of silicium was 0.005 per cent., and in another there was not a trace of silicium. It should be observed that in the presence of tin, the proportion of silicium was very liable to be over-estimated, unless special precautionary measures were taken. The first wire was the coarser one used for telegraphic purposes, and it contained 97.75 per cent. of copper, and nearly 0.25 per cent. of tin. The finer wire contained a considerably larger proportion of tin, namely, 3 per cent., and although it was examined with great care, there was no evidence of the existence of silicium at all.

Those wires certainly possessed all the properties in regard to strength and powers of elongation and conductivity which were claimed for them; but although it was probable and perhaps certain in the case of one of them that silicium might have been in some way concerned in its production, there was no positive evidence that it derived any valuable quality from the employment of that material, unless it were that, by some special treatment, silicium became in the first instance alloyed with the copper, and by its own subsequent oxidation abstracted the oxygen contained in the alloy, thus removing an antagonist to the conductivity of the material. At any rate it would appear that the electrical qualities and the strength of the wires were chiefly due to the strength of the tin alloy used, and to the metal being employed in a hard-drawn condition. Both the wires were hard drawn when he received them. The tenacity of the coarser wire was from 300 lbs. to 325 lbs; its diameter was 2 millimeters, and its weight 100.5 lbs. per mile. After annealing, its tenacity was 190 to 200 lbs.; the finer wire had at first a tenacity of from 170 to 180 lbs., and when annealed, from 95 to 100 lbs. Although it was possible that silicium might have had something to do with their production, so minute a quantity as was detected by analysis in the one instance might have been purely accidental. It was possible that the chief merit of the process for producing the so-called silicium-bronze lay in the production of a wire consisting almost entirely of pure copper in a hard-drawn condition, but with a small proportion of tin, giving it increased strength. There could be no doubt that pure copper in the hard-drawn condition, with the addition of a small quantity of tin, was a very valuable conductor, and apparently the strongest conductor of

this class with which electricians were at present acquainted.

Professor D. E. Hughes perfectly agreed with Sir Frederick Abel with regard to the use of copper as a standard. His experiments on the molecular conductivity of copper neglected everything as to its chemical composition or its form, and simply regarded the structure of the material. He had a piece of pure copper given him by Prof. Chandler Roberts which he took as a standard, and the conductivity of some of the copper deposited electrically with a Daniell cell was 225, or more than double the standard of the Mint. That value did not perhaps represent the conductivity exactly, but he would say 50 per cent. higher value. It was found that if melted, it fell 50 per cent., and if re-melted it fell again. Whatever was done to change its molecular structure the conductivity fell. He therefore maintained that electricity-deposited copper was the very best form. If a wire could be actually deposited by electricity, it would be in the best form to conduct electricity. But that was not done. It was torn, and pulled, and twisted, and was no longer copper as it ought to be, but some changed conductor. The capabilities of any material in its pure state, and therefore the real standard, were not known. It could not, therefore, be said that the standard of copper was 100. Some experiments had been made by Professor Chandler Roberts and himself on bronze and tin alloy, and it was found that in gradually making the alloys there was a certain point at which a complete and sudden rise in value took place. He wished to ask the author a question with reference to conductivity. At present, in experimenting for conductivity, they used a Wheatstone bridge, and took the measure of the electricity after it had been flowing for a minute, or three or four or five minutes, till it had arrived at a stable condition. There was a period for telegraphic purposes which had been called in France the *période variable*, which was not measured, but which was very important for telegraphic instruments when making, say, one thousand contacts per second. There was a great difference between the results with the *période variable* and the *période stable*. If the Author in measuring his wires would use a rapid interrupter, with a Wheatstone bridge having contacts two or three thousand times in a second, he would get some very valuable information of the kind that he required. He did not want to know what it would conduct five minutes after the battery was put on, but what it would conduct during the first portion of the contact. He did not agree with the Author in regard to overhead wires. As an old telegraphic engineer he remembered that the wires were constantly broken. But why did that occur? The Author had himself shown that the zinc became oxidized, and that, that particularly occurred in smoky atmospheres. Perhaps he thought that London was not very smoky, and therefore was free from danger. It was stated in the Paper that there was danger, not to the inhabitants, but to the life of the wire itself, and for that reason an effort had been made to get compounds of wire, copper and steel, which gave great promise. It was thought to be an excellent wire at first, but after four or five years its defects were discovered. Indeed the history of telegraph engineering as a whole had been to the present time a history of success; but it had also its history of failures leading on, like the Atlantic cable, to a greater success. Electricians commenced with bad conductors and bad insulators, and then they gradually improved them; but they had not yet attained perfection. The Author now thought that the silicon wire was absolute perfection, and it had the appearance of it; but perhaps in four or five years his opinion might be changed. At present he had no hope for overhead wires except in that form. He had stated that only two persons out of four hundred thousand killed by accident in thirty years had died from injuries received from overhead wires. The wires, however, had not been up during the whole of that time, having increased very rapidly of late years. But the danger from the wires was not at the beginning, it was when they were getting old and shaky; and at such time he confessed he would rather be out of London than in it. It was true that, thirty years ago, Germany, as well as England, had tried underground wires, failure being the result; but this was in the early period of telegraphy, and sufficient care was not taken in the manufacture of the cables. Since then, owing to the rapid extension

of submarine telegraphy, great experience had been gained, so that the insulation of an underground wire was far better than that of aerial lines. The German Government, after a series of exhaustive experiments, decided upon the trial of a direct underground line from Berlin to Cologne, the success of which had led to a rapid extension in all directions, there being already some 15,000 miles of underground wires. At first their electricians had some difficulty in working these lines at high speed; but in a very short time, by the adoption of the necessary electrical conditions of contacts, reversed currents, and polarized relays, they had been enabled to work their high-speed instruments with far more regularity than was possible with the constantly deranged aerial wires. After mature consideration, the French Government had also lately put their main lines underground, having some 7,000, miles which would be increased as rapidly as the finances would allow. In Paris as well as in London the telephone companies put their wires overhead; and when he was at the Paris Congress he said it was a great pity that such a beautiful city be disgraced by overhead wires; that it did not much matter, perhaps, what was done with London, but that there ought to be at least one city free from the evil. All the overhead wires were afterwards taken down. The telephone people protested and declared that the telephones would not work, and that they could not get over the induction. They were, however, forced to put their wires underground, and it was now stated that they were working better than in any other country. Berlin as well as Paris had suppressed all aerial lines in the city; and as the German and French electricians had vanquished all electrical troubles, he had confidence that the Author would equally solve the problem for England.

Mr. J. Sivewright, C. M. G., thought there could be no question as to the importance of the subject of the Paper. He could not speak from the point of view of a scientific man, or a manufacturer of telegraph wire, but he could relate his own experience as a practical man who had to do with electrical conductors, and to whom the objections to the present form of conductor had been forced home nearly every day during the last seven or eight years; and he could from that experience bring forward facts which would go far to support the Author's view as to the extreme desirability of adopting some other form of electrical conductor in preference to iron wire, at all events for aerial lines. He wished to look at the subject from the point of view of a colonial telegraph engineer, because his experience had been drawn from the colony of South Africa, with whose telegraph engineering he had been connected during the period he had named. One of the main items of expense in that colony was transport. It was almost incredible for an Englishman living in a thickly-populated country, and with ample means of conveyance at hand, to appreciate the enormous difficulties in regard to transport in a new country. It might be sufficient to say that the Coal Measures of South Africa were lying practically dormant, and it was found cheaper to import coals from Newcastle than to get them out of the bowels of the earth 100 miles from the sea-coast. Again, tens and hundreds of thousands of acres of splendid arable land were lying waste, and wheat could be imported cheaper from South Australia or from the Peruvian and Chili coasts than it could be brought from the place where the African lands were lying fallow. The cause of the difficulty was transport. The same difficulty applied to the telegraphic engineer in the constructions of his lines. If a lighter form of wire were introduced having the same electrical qualities as the iron wire employed at the present day, the transport difficulty would be to some extent overcome. It was not so much the actual saving in the expense between a coil of copper and a coil of iron wire of the same electrical conductivity, although that was considerable; the great point was that the iron wire in telegraph construction was practically the pivot round which the other elements, as it were, revolved. It was what might be called the "independent variable," according to the variations of which all the other materials employed in telegraph construction would also vary. A lighter telegraph wire meant a lighter insulator. Less surface was exposed to the insulator, and consequently a smaller insulator could be used, because there was less leakage. And not only was there less leakage, but in regard to electrical storms, the difficulties which

had to be faced were correspondingly diminished. A lighter insulator meant a lighter pole; in fact all the plant of a telegraph line would get proportionately diminished in weight according as the gauge of wire was reduced, and with a reduction in weight followed a reduction in transport, so that a very great saving was effected. The coils of iron wire would be easier to handle, and there would be increased speed in the erection of the wire, attended with diminished cost. As to the question of joints, every one concerned in the maintenance of telegraph lines knew what a bugbear joints were. Whether the wire was copper or phosphor-bronze, or silicium bronze, as long as telegraph engineers in the Colonies got light, hard, durable, non-expansive, elastic wire, they did not care. They could get longer coils of it which could be run out to the extent, probably, of 600, 700, or 800 yards, but they were obliged to confine themselves to 500 yards (100 lbs. being about the average weight of the coils); not that manufacturers could not supply longer coils, but that length was found most convenient for handling. If they could dispense with a large number of telegraph joints, they not only saved in that way, but the maintenance became a far easier matter. Those were arguments which, from the colonial telegraph engineer's point of view, he brought forward in favor of reducing the gauge in telegraph wires. One objection that had been brought against the use of copper was that being of greater intrinsic value than iron, it would be more likely to be stolen. That was a specious argument at first sight, but Europeans, at all events, who were inclined to thieve would turn their attention to something more portable and valuable, and less likely to be detected, than the telegraph wire; while the aboriginal had a superstitious and wholesome dread of the wire, so that, in times of peace at any rate, he was hardly likely to meddle with it. Until within the last two years, the South African native respected the telegraph during the war, but that respect had now gone, and amongst the difficulties of telegraph maintenance the possibility of the wire being cut and taken away had to be faced. Still he thought that a native, if he had the option, would prefer iron wire during the war to copper, because it could be more easily turned into slugs or bullets for his gun. With regard to the effects of galvanizing, he was not opposed to the process; nor would he express any opinion as to its value in countries where the same climatic conditions existed as in England. But he might mention that in South Africa a wire treated as he had described in a bath of oil was erected at least fifteen years ago; it had no galvanizing, but although they had had some splendid specimens erected since, he knew of no better wire than the one to which he had referred. He had examined a considerable proportion of its length within the last twelve months, and he could not detect any flaw or mechanical fracture. It was ordinary soft wire, and had stood well. It was in a dry climate, with comparatively little moisture, with no smoke, and with none of the chemical causes which would affect ordinary wire. What had been said by the Author as to the necessity of rigid inspection almost amounted to a truism, at least he hoped it would be accepted as such; because there was probably no branch of science in which the old proverb was more applicable, "A stitch in time saves nine," than it was in regard to the selection of telegraph materials—not only the wire but all other requisites. Perhaps it required the experience of finding, 300 or 400 miles from the seaboard, insulators open, bolts missing, porcelain gone, iron bases fractured, and telegraph wire breaking in the workman's hands, to realise the enormous importance attaching to a rigid inspection of telegraphic materials. There was not only the value of the wire, but every one knew that if a workman had good material he would make a good job, and if he had bad material he was likely to slur over it. In his opinion, therefore, any administration that neglected so vital a consideration would be guilty of an act that was not only suicidal but criminal.

Professor A. K. Huntington thought it might be of interest if he gave a few figures with regard to four specimens of silicium-bronze wire which he had recently examined. They consisted practically of nothing but copper, with the exception of a small portion of silicon and tin. In the first coil there was a percentage of 0.099 of silicon, and 0.079 of tin. In the second coil the silicon was 0.117, and the tin 0.0017. In the third coil, silicon

0.1105, tin 0.014. In the fourth coil, silicon, 0.0917, tin 0.099. In the second and third coils the amount of tin was so small as to be well within experimental error, so that he was not prepared to say whether the coils really contained tin or not. From the examination of the specimens there was sufficient evidence to lead to the supposition that a certain amount of tin was present, at any rate in two of them. Whether that tin was simply an impurity derived from the ore, or whether it was the residue of tin which had been introduced during the manufacture, he was not prepared to say, as it was impossible to arrive at any conclusion on that point from a chemical examination of the wire. Silicon appeared to be present, and taking into account the supposed process of manufacture, there is no reason to doubt the existence of a certain amount of it. He had mentioned these particulars as bearing out, to some extent, the results given by Sir Frederick Abel, but with some differences in regard to the amount of tin. With reference to the introduction of silicon, he had succeeded in introducing large quantities of that material into copper, and, in some cases, with a very remarkable effect. As to the influence of arsenic on the conductivity of copper, that, of course, was a substance which was one of the greatest culprits in destroying conductivity. But there was another which the author had not noticed, namely, bismuth, which he thought had even still greater effect than arsenic in reducing the conductivity of copper. One of the countries to which the author had referred as supplying pure copper, Australia, had ores which contained an appreciable, and, in some cases, a considerable, amount of bismuth; and as the copper was often sent in ingots, there was liability to trouble with the material so obtained; because, although the bismuth could be removed without difficulty in the "best selecting" process, due to the relative affinities of copper and bismuth for sulphur, yet in refining, owing to the copper having greater affinity for oxygen than bismuth, it was not practicable to remove the bismuth merely by oxidation, so that if much were present the quantity could be reduced but slightly. That which was removed, so far as he could make out, was due to volatilization. Now, as the copper received from Australia had not been subjected to the "best selecting" process when in a state of regulus, it followed that it would contain any bismuth originally present in the ore. He had been told by a very large manufacturer that some copper which he had received from Australia, giving by analysis 99.67 per cent. of copper, gave a very bad conductivity—something like 30 as against 90 or 100. The result had been traced to the presence of bismuth. He had found, on further inquiry, that if there was more than 0.05 per cent. of bismuth present in copper its conductivity was seriously reduced.

Mr. D. Pidgeon said that the author had mentioned only to condemn the compound conductors which had been made both in England and America, and he had condemned equally those of the past and of the present. He did not rise to gainsay this, but simply to bring before the members some examples of the compound conductors now being made in America, and to say a few words with regard to the method of plating in use there. He had had an opportunity during the last spring of passing through the manufactory belonging to the Postal Telegraph Company of New York, at Ansonia, in Connecticut, a concern which was making the same wire as that already laid between New York and Chicago. The works were originally erected by Messrs. Wallace and Farmer, of Ansonia, the well-known electricians, for the purpose of carrying out ideas which originated with Mr. Farmer, but they had passed into the hands of the Postal Telegraph Company rather more than a year ago. The works occupied a space of 250 feet square, and there were in the building two hundred and fifty plating vats, which were supplied with current by twenty-three dynamos, driven by two engines, each of 300 H. P. It would give some idea of the amount of current supplied to the vats if he stated that he saw a carbon $\frac{7}{16}$ inch in diameter and 10 inches long deflagrated in the course of a few seconds by a shunt so small that its abstraction from the general current could not be noted in the results of the plating. The plating vats were wooden troughs 20 feet long and 2 feet 6 inches either way. Over each plating trough there lay longitudinally a horizontal cop-

per shaft about 3 inches in diameter, from which there hung, like rings from a stick, the coils of wire which were to be plated—as many of them as could be accommodated in the tank, the spires being separated by slips of plate glass, so as to prevent their contact. The balance of the coils hung outside the tank over the end of the shaft. Upon rotation, the coil of wire was screwed slowly through the electrolytic bath, and, after plating, hung again from the spindle, as the unplated wire did at the other end. This operation was repeated three times, the result being that 4,000 lbs. of copper were deposited daily upon 8 miles of wire. He held in his hand a piece of the compound conductor which had been thus three times screwed through the electrolytic bath. It was 0.214 inch in diameter, had a resistance of 1¾ ohm per mile, and a tensile strength af 2,700 lbs., of which the steel contributed 1,700 lbs., and the copper 1,000 lbs. This wire was the same as that through which telephonic speech was readily heard between Chicago and New York, as readily (as he had been informed by a gentleman who listened to it) as if the length had been no greater than that of a room. It was also the wire upon which ten simultaneous messages had been sent—five each way—by means of the well-known Gray harmonic arrangement. It might be said that the wire was very expensive, if only 8 miles a day could be produced by the expenditure of such a force as he had mentioned. But the process did not stop at that point. It had been found that the cohesion between the central steel core and the copper envelope was so great that it might be passed through the ordinary wire-drawing apparatus, and the core and the envelope would be reduced together. He exhibited a wire which had been produced from the one he had already shown. This was 12-wire gauge, outside diameter, while the steel core was 18 inch Birmingham wire gauge, and on examination it would be found that the steel core was perfectly central, with the copper surrounding it equally on all sides. Here, then, was a wire the exact equivalent, electrically speaking, of No. 4 iron wire, the largest now used in telegraphy. It could be produced at the rate of 32 miles a day, and the cost, therefore, was one quarter that of the other. If that conductor came into use it would be, in the first place, because its cost was so much diminished by the fact that it could be wire drawn, and in the second place, because the wire drawing gave to both wires a considerable increase in their tensional strength, and at the same time secured the conductor against the action of those atmospheric influences which the author had mentioned in his complaint of previous compound wires. This wire, although a compound one, was as capable of withstanding the attacks of the atmosphere as that of any wire drawn from homogeneous metal. He had a specimen of the joint which was employed in the wire extending between Chicago and New York. It was a copper ferule slightly flattened, and tinned by immersion. Into it had been thrust the ends of two wires, and the whole then dipped in a bath. Economically, the wire which he exhibited was the best which the Postal Telegraph Company had yet produced; but he was inclined to doubt whether, even taking into consideration the fact that plated wires could be four or six times increased in length, they could compete with the homogeneous wires of which the author had shown such extraordinary examples.

Professor Chandler Roberts thought there could be no question that pure copper was the best material to employ, because it was well known that a very small quantity of alloying metal caused the curve of conductivity of copper to fall rapidly; and it appeared to him that any increase of tenacity that might attend the union of copper with any other metal, not excepting silver, was dearly bought by the sacrifice of conductivity resulting from alloying the copper, and, as the electrician must have copper as pure as he could get it, any improvement in metallurgical processes for the extraction of copper from its ore assumed considerable importance. There were some facts connected with the electro-deposition of copper which had not been prominently noted in the discussion. He did not think that the scale on which the copper was now deposited electrolytically was quite appreciated. There were two works on the Continent which produced annually at least 500 tons of copper, and there were two works in this country that were being fitted with dynamo ma-

chines that would enable them to deposit copper at the rate of about 60 tons per week. He did not think it was quite understood that nearly pure copper could be thus obtained from very unsatisfactory materials. To take an extreme case, there were before him some specimens containing several per cent. of arsenic, and yet, by solution and precipitation, an excellent variety of copper had been obtained from similar specimens. The author seemed to fear that electro-deposited copper did not possess the same tenacity as ordinary copper; but statements had recently been made in the continental scientific journals, especially by Austrian authorities, showing that electro-deposited copper possessed a very high ductility and considerable tenacity. At any rate, the copper was free from metallic impurities, so that even if it had to be melted the metallurgist had only to contend with the question of dissolved cuprous oxide, which, after all, was a very small matter, because it could be so easily dealt with. Copper was now electrolytically precipitated in bars, rolled directly into rods, and drawn into wire, which found a ready sale at remunerative prices; and it must be remembered that against the extra cost of the solution and precipitation of the copper by electricity there was a handsome set-off, in the shape of the precious metals recovered from the crude metal. He thought, therefore, that the question might be safely left to metallurgists, who would have no difficulty in supplying the electrician with pure copper at a remunerative rate. The author had asked him to say a few words about the deposit of iron by electrolysis. It was well known that the late Professor Jacobi and Dr. Klein, of St. Petersburg, had taught how to deposit iron of great purity, and with considerable facility. That iron, he was sure, would not compete for electrical purposes with the extraordinary soft and pure varieties of iron which could now be produced by ordinary metallurgical means. The author had alluded to the effect produced by silicon in alloy with copper, and it had been stated that $\frac{1}{100}$ per cent. of silicon might be retained by copper. He should like to mention, because he did not think it was very clearly understood, that silicon had an extraordinary effect on many of the alloys of copper. The ordinary alloy of gold and copper containing 90 per cent. of gold fused at about 940° Centigrade, and, if it contained only $\frac{15}{100}$ per cent. of silicon, its melting point was so much reduced that a strip would bend and fuse in the flame of a candle. Hence it was that the presence of a minute quantity of silicon might be of the greatest importance in modifying the physical constants of pure copper.

He spoke with very little knowledge of the ordinary methods of electrical testing, but he did not think that the extreme delicacy of the induction-balance of Professor Hughes was quite recognized. He had some specimens of copper, which in regard to analysis were identical, and yet when disks of each sample of metal were treated in precisely the same way, mechanically and thermally, they showed a wide difference when submitted to this beautiful instrument, which would be a powerful ally in future, both of the electrician and the metallurgist. The principal point, however, to which he wished to direct attention was, that the metallurgist could supply the electrician with pure copper at a reasonable cost.

Mr. W. Carson desired to say a few words with regard to the effect of the presence of manganese, even in small percentages in conductors, upon the conductivity of the material. He had obtained two samples of puddled iron, from each of which he had made a number of tests. The first sample was from a highly manganiferous pig, with 6 per cent. of manganese. The second contained 3 per cent. of manganese. On being puddled, made into rods, and drawn into wire, the highly manganiferous pig produced an iron containing 98.871 per cent. of metallic iron, and 0.49 per cent. of manganese. The other sample of iron containing 3 per cent. of manganese gave 98.89 per cent of metallic iron in the wire, and only 0.184 of manganese, yet the mile-ohm resistance had risen to 5921.8. Under those circumstances he hardly thought that the Author was justified in laying the whole blame upon the manganese. He did not think that the manganese had much to do with the result.

Mr. J. Thewlis Johnson said the Author had shown the general improvement in the conductivity of copper wire, and had intimated that the improvements in iron wire had been forced on the makers,

who had not sufficiently availed themselves of the services of the chemist. In 1857 a relative of his, with the late Dr. Calvert, made a great many chemical experiments on pig iron, especially as to the changes in pig iron when being converted into wrought iron for wire purposes. The notes were published in the "Philosophical Magazine," in 1857, but the pecuniary results had been very small. Since then some of his friends had, like himself, taken a great deal of pains in conducting chemical experiments, but as far as his own results were concerned they had not been very considerable, and he had come to the conclusion that no iron wire could be made giving a less resistance than what was at present obtained. The difficulty of the iron-wire manufacturer had been to produce a material which would yield certain mechanical and electrical tests. The specifications recently drawn up were a great improvement on those existing twenty years ago, and the examinations were conducted on a rigorous and fair system. He thought it was generally admitted in the trade that manufacturers all knew what wire would give certain results, and the examining officer in making his tests soon found whether he would be able to pass the wire or not. When he had made a few tests he knew pretty well that if the remainder of the wire was of the same character it would either be all passed or all rejected. No one more than the Author had impressed upon wire manufacturers the necessity of endeavoring to improve the conductivity of iron. In season and out of season he had urged it upon them, and the result was that they were now all producing an iron which gave, ton for ton, a better average of conductivity or a less resistance than was obtained twenty years ago. But he wished to direct attention to the fact that while it was perfectly true that the iron wire used in England had decreased in resistance from about 15½ ohms in 1873 to 11¼ in 1883, low resistance wire had been a regular article of commerce long previously. He knew that in 1862 some 2,000 miles of wire were sent from England to the French Telegraph Administration, and it was identically the same as that now used by the English Post Office. In the specification of the French Telegraph Administration in 1862 there was no stipulation as to the conductivity of the wire; but it was stipulated that the wire should be manufactured in a particular way, which gave certain high mechanical tests, and also the conductivity tests which the Post Office now demanded. He could give many other instances, but he would only mention the line crossing the continent of Australia, erected in 1871. It was an unfortunate thing for the credit of England that the introduction of the conductivity test in the specifications, although advocated so long by the Author, did not first take place in England, but in America, by the Western Union Telegraph Company. He hoped the members would not leave with the impression that the days of iron wire as an electrical conductor were numbered. He had listened with great interest to the remarks on the process of depositing copper on steel, and he had been astonished by the statement that during the process of drawing wire when the copper was deposited on the steel the diameters of the steel and the copper were reduced equally. But for that statement he should certainly have doubted the fact. He believed that in practice it would be found that the deposited copper-coating would occasionally break in the process of drawing, and further, that as wire when drawn down became harder, and had to be annealed before the process could be repeated, a difficulty might arise in annealing a compound wire composed of steel and copper. The experiments with phosphor-bronze wire and compound wire, according to the Author, had not resulted satisfactorily, and there were only two other conductors, both of which were so expensive that economy compelled them to be used with a very small diameter. It was asserted that No. 14 copper wire, costing £87 a ton, was as efficient a conductor as No. 8 iron wire, costing £20, and that, mile for mile, it was not more expensive. That might be so, but he questioned whether it would be found as economical in practice. He thought that when it was scattered all over the country a good deal would be stolen. At Vienna there had been a large exhibit of silicium-bronze, the cost of which he was told was £162 a ton. It appeared to him that it would make a very charming toy line. It was never used till November 1881, when the first quantity was sent by the French

manufacturer to the Austrian Telegraph Administration for an experiment. He had seen no statistics as to its life. In India, monkeys often sat on the wires, and in the Colonies the birds flew against them, and he did not see, under such circumstances, how telegraph wires were to be maintained if they had only the diameter of pins and needles. Whatever conductors were employed, he thought they ought to have a good substantial diameter if the lines were to be maintained for any length of time.

Mr. A. J. Bolton exhibited some samples to show the process of manufacturing copper wire. The Author had referred to the rapid strides which had been made in improving the quality of conductors for electric currents, but these had also been greatly improved in form. It was often a great advantage to the electrical engineer to have the conductors of great length, and that object had now been accomplished. In 1850, when Mr. Charlton J. Wollaston, the electrical engineer of the experimental cable laid between Dover and Calais, came to his office with the order for the copper wire for the core of the cable, he was imperative that it should be made in continuous lengths to weigh about 30 lbs. in each piece. At that time, the only wire produced in copper of that size weighed about 4 lbs. in continuous length; and the foreman who was sent for to receive instructions, when he heard what was required, said, "Does the man think I am a fool?"—so impossible did he consider its production. At the present time continuous lengths were commonly made, with copper wire of the same diameter, weighing 70 or 80 lbs. The Author had stated that conductors for cables and electric light mains should be constructed with the purest copper producible. That object had been attained, and the samples on the table tested over 99 per cent. by Matthiessen's standard. With regard to aerial lines there might be some question. In his own works it had been found that in attempting to improve the tensile strength by introducing any foreign substance into the pure copper there was a loss of conductivity. The 14-gauge hardened-copper wire with high conductivity gave a breaking-strain of 340 lbs., and a resistance of only 8 ohms to the mile; the weight per mile was 103 lbs., and the cost about £4. A wire, 15 gauge, which had a resistance of 10¾ ohms per mile, had a breaking-strain of 255 lbs., and the cost was only 65s.—about the same as that of ordinary iron telegraph wire 0.171 size.

It would be seen from the table of the Author's experiments with silicious bronze that the 0.081 wire, with a resistance of 8½ ohms per mile, gave only about an equal breaking strain to that of hardened copper of the same gauge, and the cost was about £6 per mile. It appeared, therefore, that hardened wire of high conductivity answered all the ends, and the breaking strain was a very good one, 340 lbs. being sufficient for all purposes. The objection to the introduction of phosphorus or other materials was, that results were never uniform; but with wire of high conductivity the results were always exactly the same. When silicon was introduced conductivity was wanting. The Author's next requirement was a low inductive capacity. That, he believed, would only apply to cables or insulated wires placed underground. Next, that the conductors should expose to wind and snow the least possible surface. Of course the lower the resistance the less the diameter of wire necessary to pass the same current, and therefore the wire of high conductivity would seem best to accomplish that end. The last requirement was that the wire should be practically indestructible. In that respect he thought that copper had everything in its favor. He might instance the case of a wire that had been more than twenty years over his own works; it had been constantly used for the passing of electric currents, and it had not altered in its conductivity or in its diameter during that period. As to coating steel wire with copper, and then drawing it down to make a conductor, reducing the diamater both of the steel and the copper, he too was much astonished. In his own works it was a common thing to put a steel mandrel inside a copper tube, and to draw the tube down; but the mandrel always remained the same diameter. He could hardly understand how the result that had been stated could be possible. To get wire of high conductivity it was essential that the ores, or the copper bars imported from Chili should contain as few alloys as possible, as then there was not much oxidizing required in the refin-

ing process. The copper was cast in large bars (of one of which he held in his hand a section). These were rolled into long strips like the specimen exhibited, and then passed through a series of circular steel cutters, which cut them longitudinally into narrow pieces, each of which was passed edgeways through grooved rolls, forming a nearly circular bar of copper, which was subsequently drawn down to wire of the required size. The process was different from the manufacture of iron wire, which was not slit but rolled down into a thin rod from a large bar. He also exhibited some specimens of hardened copper wires which had been subjected to different tensile strains, showing the diminution that had taken place in size, but they generally secured a breaking-strain of 340 lbs., which was found to be practically sufficient.

Sir Frederick Bramwell, Vice-President, said it appeared Mr. Bolton, like Mr. Johnson, was surprised that the two metals in a steel wire, coated with copper, when drawn should draw uniformly, and in support of his doubts had referred to the fact that, if a steel mandrel were put inside a copper tube, there was no elongation of the mandrel when the two were drawn together. That was perfectly true; but it should be remembered that in that case there was no metallic attachment of the mandrel to the tube. He wished to remind the meeting of a cognate instance in rolling—that when lead and tin were cast together in two thicknesses, to make Dobb's metal—it was formerly alleged that these two metals would not roll out uniformly; but, as a matter of fact, in whatever proportions they were cast—3 to 1, or 4 to 1—the rolled-out sheet showed the same proportion, although the metals were so dissimilar in their powers of yielding under the roll.

Mr. James Rock said he should like, as representing a company which manufactured both phosphor bronze and silicium-bronze, to say a few words on the subject under discussion. With reference to the uniformity of manufacture of alloys of copper, especially with silicon and phosphorus, it was part of the process of his company to manufacture those metals in strictly uniform proportions, and he could answer for the uniformity of the products as well as Mr. Bolton could for that of the copper which he had produced of such excellent quality. The French manufacturer of silicium-bronze, who was also the inventor, supplied large quantities to the French and other Continental Governments, and he guaranteed and produced it of certain degrees of conductivity and tensile strength. So far from silicium bronze being at present only a toy, on the Continent it was interfering largely with the use of iron wire for telegraphs. That was not the case in England, because the material was comparatively new. The analyses of Sir F. Abel and Professor Huntington confirmed the analyses in his own possession of similar alloys. There was, however, an apparent discrepancy with regard to the quantity of tin in the specimens. It was part of the process of manufacture that the proportions of tin and silicium should be adapted to the special uses for which the wire was intended. They accordingly produced two special kinds of electric conductors, one of high conductivity for telegraphs, and one of lower conductivity, but much greater strength, for telephones. The wire for telephones was largely used in England. It had very great tensile strength—as great as that referred to by the Author in connection with the telegraph wires for the Mumbles. It had similar properties to those wires, but a much higher conductivity; instead of 20 per cent., it had from 36 to 40 per cent. The apparent discrepancy between Sir F. Abel and Professor Huntington was probably due to their having analyzed examples of different constitution, one the telephone wire, and the other the telegraph wire. It was possible to produce silicium-bronze of different degrees of conductivity and of tensile strength. If electricians and engineers would only say what amount of strength and conductivity they required, the proportions could be adjusted accordingly. The product was a very valuable one, and the analysis of one specimen should not be taken as representing silicium-bronze generally.

Mr. Frederick Smith observed that the manufacturers of iron wire had not failed to make use of the services of chemists in endeavoring to improve that material. It should be borne in mind that the manufacturers of copper had a very dif-

ferent material to deal with at the outset, and the same advance therefore ought hardly to be expected from iron-wire manufacturers. Great progress, however, had been made, and he thought that the resources of civilization were not yet exhausted even in the improvement of iron or steel wire. He had been testing some samples of an English made steel wire, which gave as high a degree of conductivity as those of any Swedish charcoal iron he had tested, together with a much greater breaking-strain, and a higher degree of torsion. The six samples yielded the following results: Diameter 209, resistance, 8.13 ohms; diameter 208, resistance, 8.79 ohms; diameter 208, resistance, 7.98 ohms; diameter 209, resistance, 7.23 ohms; diameter 209, resistance, 7.74 ohms; diameter 209, resistance, 7.98 ohms. The samples were all tested in the bright hard state previous to galvanizing. After galvanizing, the improvement would be about 0.3 ohm per mile in each case. That was practically a new material. All who had worked for the specification for which the wire was used would know that a practically new material had to be made in order to meet the requirements. Instead of taking a material that already possessed a high degree of conductivity, the manufacturers had to take a material that gave a very great degree of resistance, and to bring it up to a high degree of conductivity; and they could only do that by making use of the services of chemists. With reference to the adoption of copper and phosphor-bronze or silicium-bronze, it should be borne in mind that a telegraph wire had a double function to perform. If it was simply a conductor and had only to carry the current, the wire with the highest degree of conductivity would undoubtedly be the best to employ; but the wire had not only to carry the current, it had to carry itself; it had to be out in all weathers, over very long distances, through all kinds of country, and it had to encounter all kinds of risks. It was necessary, therefore, that it should have a certain amount of bulk. Much had been said about wires breaking down, but if wire was used of the kind that had been so highly praised, he thought the number of breakages in all parts of the country would be greatly increased. He had recently tested a sample of hard-drawn copper wire, and had found that the mechanical properties were inferior; it would not stand wrapping and unwrapping round its own diameter without breaking. That was a test which all hard wires ought to stand. The material bore only about ½ per cent. elongation, which an inspector would put down as nil, after allowing for the fracture itself. The breaking strain also was very slight, and it would only allow fourteen twists in 6 inches. The size of wire that he tested was 0.81, about No. 14 W. G., which was about one-quarter of the weight of iron wire of the same conductivity. Instead, therefore, of being only four times the cost, as might be expected, it was eight times the cost, consequently the cost of the line made of copper would be twice that of a line made of iron wire, so far as the wire was concerned. It was also exposed to great risk from the slightest motion of the posts, and any substance thrown against it caused it to break immediately. In hot countries, where perhaps the wire would be most used in consequence of the great cost of transport, the results of changes in temperature might be very serious. The sun was very hot by day, and the nights were often cold, and the wire would not possess sufficient ductility to stand such changes in temperature, which might cause it to snap. Mr. Johnson appeared to think that in the manufacture of compound wire, the steel would not be reduced at the same time as the copper or to the same extent. He might be permitted to remind him that it was the custom amongst wire-drawers to draw wire coated with tin, and both were reduced at the same time without the tin being removed by contact with the draw-plate. If so soft a metal as tin could be so treated he thought that copper would bear the same kind of treatment.

Mr. J. H. Greener had been connected with telegraph works for nearly forty years, and most of his time had been spent in erecting lines and supervising stores for different administrations. Meanwhile great improvements had been made on all sides. He was glad of the opportunity of speaking of iron wire before it became a thing of the past, which seemed to be the hope of the manufac-

turers of copper and bronze. Twenty years ago iron wire weighing 600 lbs. to the mile would not bear the strain of 1,000 lbs. without breaking. In the present day, with the same wire a breaking strain of 2,000 lbs. could easily be obtained. The Post Office specification required a wire of very fine material, almost pure iron, because, working as they did duplex and quadruplex, the authorities wanted high conductivity. The specification for the India Office required for a wire of the same size 350 lbs. more breaking strain. The extra breaking strain had been a great bugbear to the wire manufacturers. They could easily give the 350 lbs. additional breaking strain, and also the capability of sustaining fifteen twists in a length of 6 inches, but when the conductivity test was also applied they had a very difficult task to perform. It had been accomplished, but to effect this the aid of the chemist had to be called in, because the wire manufacturers found that it would not pay to go on with the rule of thumb methods previously in use. He had with him some tests of the wire made by different firms during the past year. In the wire made by No. 1 firm, the average breaking strain was 2,196 lbs.; No 2, 2,123 lbs; No. 3, 2,150 lbs.; No. 4, 2,195 lbs. This uniformity was astonishing. Those results had not been achieved without a great deal of hard work, worry, and anxiety on the part of the manufacturers, who, in carrying out the specifications, had always been willing to render every possible assistance. No doubt they had often thought him a great nuisance, but they had certainly helped him in every possible way.

Mr. Killingworth Hedges desired to say a few words on the effect of powerful lighting currents through a copper conductor. It was most important that engineers who had to do with electric lighting, and local authorities who proposed to purchase expensive copper cables and lay them down to distribute electricity in their districts, should be certain that the passage of a powerful current had no effect on the conductivity of the wire. He had noticed, as mentioned by the Author, that wires taken from dynamo machines, especially those which had an alternating current passing through them, became brittle. He had thought it might be interesting to ascertain whether there was any change in an ordinary commercial cable which had been carrying a current of electricity for some time, and he had for this purpose tested a cable through which a current had been flowing for nine months, say about sixteen hundred working hours. The electromotive force was 1,000 volts, the size of the cable 7 strands, No. 16 B. W. G., and the current 10 amperes. He tested the resistance carefully, and compared it with that of a cable of similar design which had never been used, and he could find no difference. He then had the fracture examined microscopically, and he could detect no signs of brittleness. The cable had only been working a comparatively short time, and the current therefore was very small in proportion to the area, about one-fourth of what the maximum current might have been as allowed by the Board of Trade, 2,000 amperes per square inch of section. If a larger current had been used there would probably have been some heating, as Dr. Matthiessen, in his contribution to the Royal Society,* had shown that if heating took place in a copper cable of a certain degree of impurity, there was an alteration in its conductivity, and he had quoted the case of a very impure piece of copper, which increased in resistance 0.064 per cent., after being heated to 100° Centigrade for three days. Dr. Matthiessen's experiments only went as high as 100° Centigrade, but Mr. Hedges thought he would go as high as he could in heating various wires of pure metals and alloys by a current of a secondary battery, and the results were stated in the annexed table.

The first column showed the material used; the second, the resistance before heating; the third, the decrease of resistance after heating; and the fourth, the approximate temperature as nearly as it could be obtained. The experiments tended to show that the pure metals underwent no change in resistance after being heated twenty-four hours; but those containing some commercial impurity decreased in their resistance and increased in conductivity. The decrease was very small and was most noticeable in No. 3, the tin alloy, to which he desired to

* Phil. Trans. 1864, vol. 154, p. 167.

| Materials. | Resistance. | | |
|---|---|---|---|
| | Before Heating. | Decrease after 24 Hours. | Temperature. Fahrenheit. |
| No. 1. Tin wire | 0.815 | 0.003 | 390 |
| No. 2. Lead | 0.835 | 0.005 in 3 hours | 590 |
| No. 3. Tin alloy | 0.870 | 0.005 | 490 |
| No. 4. Copper | 0.810 | No change | 1,900 |
| No. 5. Tin-foil | 0.860 | " | 400 |
| No. 6. Albo alloy | 0.835 | " | 1,200 |

draw attention. The experiment in that case was only carried on for three hours, by which time the conductivity had increased 7 per cent. He could not carry it on longer because the alloy melted, probably by the decrease in resistance allowing a larger amount of current to pass. This alloy (tin and lead) was well known in the form of solder, which was largely used for jointing electric-light wires. It was dangerous to rely on solder alone because it would be softened by the passage of the current if there was any heat caused by a slightly imperfect joint, and an action would then go on which he thought had a tendency to reduce the metals into their constituent parts, so that by degrees the two wires would come a little apart, an arc would be formed, and the joint destroyed. All joints for electric lighting purposes ought to be made mechanical under considerable pressure, the solder should only be used to keep away the air. It would be interesting if the Author would give his experience of the joints used by the Post Office, also whether there had been any variation in resistance after they had been employed a long time. The question of joints was most important for electric lighting. Dynamo machines afforded a most economical method of transforming mechanical work into electricity, and the problem of using the current of electricity and distributing it successfully appeared to him to be in the cost and the efficiency of the conductors and their joints. Now it was required in a perfect conductor that the current in its passage should develop no heat until it arrived at the lamps, where the heat would be utilized in the form of light. The Author had suggested a possible variation of resistance due to temperature of 20 per cent., between summer and winter. He hardly thought that would be the case in England. In some climates perhaps there might be that amount of variation if overhead wires were used for electric lighting purposes, but if they were placed underground he did not think they would be subject to as much variation in temperature as were ordinary gas and water pipes, in that they would be surrounded and carefully insulated by some material which not only was an insulator of electricity but a non-conductor of heat.

Mr. T. H. Blakesley considered that in view of the large outlay which would probably take place in conductors for the distribution of electricity for electric lighting or other purposes, the Author had passed rather hurriedly over the law of economy which Sir William Thomson was the first to suggest. He ventured to recall the attention of the Institution to that point, first because of its great importance, secondly because the Author had somewhat imperfectly quoted the law, and thirdly because that economy was in practice very rarely carried out, it being generally thought advisable to save present outlay at the expense of future current expenses. The problem of course, was analogous to the one which engineers constantly had to encounter in pumping through hydraulic mains. If the main was contracted too much, there was a saving in the expense of pipes but a loss in the current expense of pumping. There were two forms of the law usually given, one that which had been stated by the Author, and the other asserting that the current should be always proportioned to the sectional area of the conductor. Both statements were somewhat imperfect, and were, moreover, very often misapplied. Sir William Thomson arrived at a result

giving about 322 amperes to the square inch of conductor of copper, when the price of the copper was £70 a ton and 5 per cent. interest was allowed on the outlay, and the time during which the current flowed was one-half, or twelve hours a day. The late Sir William Siemens, in his address before the Society of Arts, arrived at the value of 390 amperes per square inch, taking £90 as the value of copper per ton and 7½ per cent. interest on the outlay, and one-third of a day, or eight hours in twenty-four, as the time of working. All these changes tended in the same direction. Now 390 did not materially differ from 324, and even one of the alterations which he made was sufficient to account for the difference. The reason why the divergence was so small was that he had put the cost of energy at 12d. per 10 Board of Trade units, which most people thought very high. He had used what would correspond more to the price of energy than to the prime cost; but in that problem no doubt the prime cost was the thing that should be taken into consideration. But whatever the values of these quantities, Sir William Thomson's law was only applicable to conductors where the expense of the conductor varied with the weight of it. No doubt in telegraph wires that was approximately the case; but in the distribution of electricity on a large scale for electric-lighting purposes, or for the transmission of energy, the conductors were insulated, and probably underground, and on comparing the price of a bare conductor with its cost when highly insulated, it would be found that the price of the insulation was in excess of that of the conductor itself, for ordinary conductors, say of a ¼ square inch section, and that the extra expense of insulation did not vary anything like according to the section of the conductor, but more nearly as the diameter of the wire, or as the virtual diameter in the case of a bundle of wires. That would greatly modify the law as usually quoted for a proper economical system. One outcome of the calculation in this case was that there was no longer the constancy between the current to be carried, and the sectional area to carry it, which was so often quoted. Another outcome was that the number of amperes that might be put through a square inch was much higher than that given by Sir William Thomson or by Sir William Siemens. He had ascertained from the prices of a well-known manufacturer, that, with a conductor thoroughly insulated with gutta-percha and covered with jute and tape in sections ½ square inch, and employed for twelve hours in twenty-four throughout the year, there ought to be 566 amperes per square inch passing through it; for less sections of conductor more could be put through a unit of section. This rate per square inch increased sensibly, though not largely, as the section got smaller.

Mr. W. H. Preece, in reply, observed that Sir Frederick Abel questioned the difference said to be found between the conductivity of hard-drawn and soft copper; but his own figures showed that it was 2 per cent. in favor of the latter, and this corroborated the accepted figure. He still maintained that iron manufacturers had not sufficiently brought to their aid the talents of the chemist. At the present time they absolutely did not know what was the conductivity of pure iron. Professor Chandler Roberts had kindly offered to supply him with a piece of pure iron, but it was not certain that he had a piece to supply. Indeed, he did not know that a piece of pure iron had ever been seen. If anybody would supply electricians with absolutely pure iron, they would be delighted; at present they did not know what resistance it gave. It was true that Professor Hughes, with his exquisite little instrument, had told some curious stories about the internal arrangement of molecules, and he was prepared to assert that certain specimens were 225 times better than certain other specimens. He had not mentioned, however, how to obtain iron of sufficient purity to make perfect magnets and perfect wires. On the question of iron bars and copper wires, Professor Hughes had suggested that the probability was that after five years' experience copper wire would be rejected, just as compound wire had been. In England the requisite five years' experience had been obtained; and electricians certainly had not arrived at the conclusion that copper was a better conductor for overground purposes than iron, without long experience and very rigid practical tests. They now had copper wire, not in pounds or in miles, but in hundreds

of miles scattered over the country, and the result of their experience had been to show that for certain purposes (he did not say for all) copper had a greater superiority over iron than some of the speakers would be inclined to believe. Mr. Johnson appeared to think that copper would be stolen, but that had not been the result of past experience. As to monkeys climbing upon wires, those animals, like iron-wire manufacturers, had increased in intelligence. In the early days of telegraphy they did climb upon the wires and break them down, but they did not do so now. Perhaps the difference was due to the fact that the first telegraph wires were made of thick iron rods, while the more recent wires were much finer, and the finer they were the less likely monkeys might be to perform gymnastics upon them. He was sorry that Professor Hughes differed from him on the subject of overhead wires. He believed that all electricians were agreed upon one point, that whatever the merits or demerits of overhead wires, they were better adapted for high-speed apparatus and general telegraph work. There could be no doubt that if a wire were extended between London and Manchester it would be commercially sixteen times worse underground than above ground; and to try to force an Administration like the Post Office to put its wires underground, because of some imaginary danger, was as absurd as it would be to try and force every railway company to tunnel underneath the roads instead of crossing them by bridges. It had been said that overhead wires were dangerous, but, as he had asked before, where was the danger? There was much more danger in opening trenches. The number of accidents from the latter cause very far exceeded those from falling wires. As to the question of economy, those who had to spend the money were, he thought, the best judges on that point. His remarks, however, applied to outlying districts. The parish of Wandsworth, for instance, wanted a fire telegraph and a police telegraph; the Post Office authorities wished to put the wires overhead, and the Wandsworth Board tried to force them to put them underground: the postal authorities had resisted, and the matter was now before a court of law. The opposition raised in many of the outlying districts against overhead wires was, he considered, a senseless proceeding; it meant additional expense, and it did not remove danger. Who cared about the appearance of wires in a country district? No doubt many of the streets in London would be considerably beautified if some of the numerous wires were removed; his remarks, however, did not apply to the city of London, but only to outlying districts. What local authorities needed was power to control the erection of overhead wires, not necessarily power to forbid their erection. In all large cities and towns the Post Office put the wires underground, not to diminish danger, nor to improve appearances, but for convenience and economy; they had even an underground line between Liverpool and Manchester. Wherever it was necessary the lines were put underground, but they did not intend to put them there unless it was absolutely necessary, and they were not to be forced to do so by any absurd mania on the part of the local authorities. The important question of joints had been raised by Mr. Hedges; but there was one that had been settled long ago by telegraphists, it was, that the telegraphic joint as now constructed was perfect. In his long experience of thirty years, and he could appeal to those who could speak of a still longer period, the existence of a bad joint in a line properly constructed by an experienced man was absolutely unknown. He had seen, however, in lines used for electric lighting, most inefficient joints, and it was a matter of surprise that many of the electrically lighted public buildings had not been destroyed by the utterly unsuitable wires that had been employed. There had been serious accidents, and there would be more unless electric-lighting engineers took a little more to heart the lessons of experience on this point. He had been contented with quoting in its broadest sense Sir William Thomson's law, leaving the results to be worked out by those who wished to apply it, and who possessed the current prices of materials. The subject had been more fully worked out by Mr. T. Gray. He however agreed with Mr. Blakesley, that the number of amperes per sectional area which could be transmitted with safety and economy was greater than that given by Sir William Thomson and the late Sir W. Siemens.

# EXAMINATION OF THE FORMULÆ USED IN CALCULATING THE PRESSURES IN STONE WORK.

By E. SHERMAN GOULD, C. E.

Written for Van Nostrand's Magazine.

In an article published in the April No. of this Magazine, I gave the two fundamental formulæ used in determining the maxima elementary pressures upon the base of a dam or other structure of masonry as follows:

$$(1) \qquad p=2\left(2-\frac{3u}{l}\right)\frac{P}{l}$$

$$(2) \qquad p=\frac{2}{3}\cdot\frac{P}{u}$$

These formulæ are of the utmost importance; they constitute the foundation of the entire theory of high masonry dams. They were given in the article named without demonstration—their general acceptance by the highest mathematical and engineering authorities seemed to render anything beyond their mere enunciation unnecessary. Enquiries and criticisms which have since come to my notice, however, suggest that some further examination of them may prove of interest.

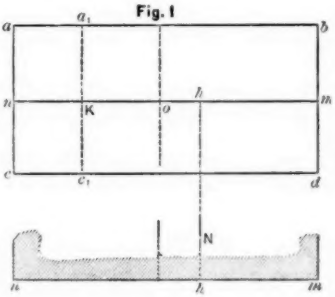
For this purpose I cannot do better than present a translation of Debauve's investigation and discussion of these formulæ, adding a note here and there which may be useful to those who approach the subject for the first time.

Mons. Debauve treats of the matter in his work upon stone bridges (10th fascicule of the "Manuel de l'ingénieur") under the head of abutments. I proceed to translate, commencing at page 109 of the above.

Problem.—Being given a homogeneous mass of masonry (Fig. 1) with vertical sides and a rectangular horizontal section *abcd*, the base being pressed by a vertical force N, the point of application *h* of which is situated upon one of the axes of symmetry of the rectangle, it is required to determine the elementary pressure at any point of the rectangle, and particularly to determine that point at which such pressure is maximum.

Note.—*By "elementary pressure" is meant the pressure per square unit, expressed in any given standard of weight. Using French measures, the elementary pressure would be given in kilos. per square meter, which would, however, be generally reduced to kilos. per square centimeter. Using English measures, it would be given in lbs. per square foot, usually reduced to lbs. per square inch. By "one of the axes of symmetry" is meant the line nm* (Fig. 1) *which divides the rectangle into two equal parts.*

Fig. 1




In chapter I. of his "Cours de méchanique appliquée," Mons. Bresse solves completely the above problem by a consideration of the centers of percussion. We cannot here present this elegant and interesting investigation. The results reached by Mons. Bresse are given at page 62 of his treatise, and are as follows:

1° When the point of application *h* of the vertical pressure N is such that its distance *oh* from the center of the rectangle is less than one third of the half axis *om*, if we represent the ratio $\frac{oh}{om}$ by $n$, and the surface of the rectangle *abcd* by S, the maximum pressure will occur at the edge *bd* of the rectangle, and this pressure per unit of surface will be given by the formula

$$(1) \qquad p=\frac{N}{S}(1+3n)$$

2° If on the contrary the ratio $\frac{oh}{om}$ is greater than one third, the maximum pressure still occurs at the edge *bd*, but its degree is given by the formula

$$(2) \qquad p=\frac{N}{S}\cdot\frac{4}{3\,(1-n)}$$

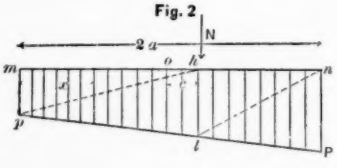
In the first case the entire surface *abcd* sustains pressure; when the ratio $n$ becomes $\frac{1}{3}$, the entire surface is still pressed, but the pressure is *nil* at the extreme edge *ac*; in the second case, (when $n$ is greater than $\frac{1}{3}$), if we take $mk=3mh$ the pressure will be *nil* beyond $a_1c_1$ and will go on increasing from $a_1c_1$ to *bd*.

NOTE.—*It will be seen that the above formulæ are really identical in value with those quoted at the commencement of this article, though under a different form. To establish their identity, it is only necessary to replace* N, S *and* $n$ *by their equivalents* P, $l$ *and* $\frac{l-2n}{l}$. (*bd being supposed to be equal to unity,* $S=l$).

But a simple demonstration may be given of the preceding formulæ.

Let us represent the length of the rectangle by $2a$; by $p$ and P the pressures at $m$ and $n$ (Fig. 2), and let $x$ be the distance separating $m$ from any given point in the axis $mn$, and $c$ the distance between the point $h$ of application and the center $o$ of the axis.

Fig. 2




Under the influence of the pressure N, the base $mn$ is more or less displaced (or compressed). We assume that it does not become distorted but remains a straight line. We assume, moreover,—and this is in accordance with the laws of elasticity—that at any given point the elementary pressure is measured by the degree of displacement or compression. Thus at $m$ the pressure is measured by $mp$.

In virtue of these hypotheses, which experience in a measure confirms, we may calculate the compression by imposing the condition that all the elementary pressures have a resultant equal and opposite to N. For this it is necessary:

1° That the sum of the elementary forces such as $mp$,—$a$ sum represented by the area of the trapezoid $mnp$P,—should be equal to N. This furnishes the equation:—

$$(1) \qquad N=(P+p)a$$

2° That the sum of the moments of the elementary forces about the point $h$ should be algebraically equal to $o$. (Fig. 2).

The sum of the moments of the forces to the left of $h$ is equal to the moment of the area of the trapezoid $mhpl$, and that of those to the right of $h$, is equal to the moment of the area of the trapezoid $nh$P$l$. The vertical $hl$ is equal to

$$p+(P-p)\frac{a+c}{2a}$$

NOTE.—*Put* $hl=p+z$. *Then* $z$ *is given by the proportion*

$$\frac{z}{P-p}=\frac{a+c}{2a};\ z=(P-p)\frac{a+c}{2a}$$ *and* $hl$ *is found to be as above.*

The altitude of the left hand trapezoid is $(a+c)$, and that of the right hand trapezoid is $(a-c)$. Putting the moments of the two trapezoids in equation, we obtain:

$$(2)\ p.\frac{a+c}{2}.2.\frac{a+c}{3}+\left(p+(P-p)\frac{a+c}{2a}\right)\times\frac{a+c}{2}.\frac{a+c}{3}=$$

$$=P.\frac{a-c}{2}.2.\frac{a-c}{3}+\left(p+(P-p)\frac{a+c}{2a}\right)\times\frac{a-c}{2}.\frac{a-c}{3}$$

NOTE.—*It will be observed that the two trapezoids are divided into four triangles by the lines* $ph$, $ln$ *and it is the sum of the moments of these triangles which is given in* (2).

From equations (1) and (2) the following values of $p$ and P are derived, in terms of N.

$$(3) \qquad P=N\frac{(a+3c)}{2a^2};\ p=N\frac{(a-3c)}{2a^2}$$

NOTE.—*To obtain these equations, put* $A=(a+c)$ *and* $B=(a-c)$. *Then by re-*

*duction, simplification and substituting the value of* $P=\frac{N-ap}{a}$ *from* (1) *we have;*

$$2a\,(3A^2a-A^3+aB^2+AB^2)\,p=$$
$$=N\,(4aB^2+AB^2-A^3)$$

*Replacing* A *and* B *by their values, effecting the necessary calculations and reducing, we have* $p=N\frac{(a-3c)}{2a^3}$ *as above.*

*The value of* P *is then obtained by substitution in* (1).

From (3) we see that the maximum pressure occurs at the edge of the rectangle nearest to the point of application of the pressure N.

Since the above calculations refer to a rectangle *abcd* of which the width *bd* is supposed to be unity, we have $S=2a$. If now we put $\frac{c}{a}=n$ the value of P may be written under the more simple form given by Mons. Bresse;

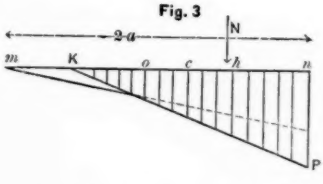
$$P=\frac{N}{S}\,(1+3n)$$

Let us now see how the elementary pressures vary as the force N changes its point of application; $c$ may vary from $o$ to $a$.

For $c=o$, we have $P=p=\frac{N}{S}$. That is, the pressure is uniformly distributed over the whole base.

As $c$ increases, P also increases, and $p$ diminishes. When $c=\frac{a}{3}$, $p=o$; so that for $c>\frac{1}{3}\,a$ there is an area $mk$ (Fig. 3) of the

Fig. 3




rectangle which sustains no pressure, and this area is easy to determine, for it is only necessary to take $nk=3nh$. At $k$ the pressure is nil, as it is also for all the surface situated to the left of $k$, and the pressure increases progressively from $k$ to $n$, where it becomes maximum.

For $c=\frac{a}{3}$; $P=2\frac{N}{S}$

that is, the pressure is double that which occurs under an uniform distribution.

For $c>\frac{a}{3}$, we have (Fig. 3) $nk=3\,(a-c)$ and the area of the triangle $knP$ which represents the sum of the elementary pressures, should be equal to N, whence we have the relation:

$$N=\frac{P}{2}\,.\,3\,(a-c),\text{ or }P=\frac{2}{3}\frac{N}{(a-c)}$$

Put $\frac{c}{a}=n$, and remembering that the surface of the rectangle forming the base is measured by $2a$, we may write the value of P thus:

$$P=\frac{2}{3a}\cdot\frac{N}{1-n}=\frac{4N}{S}\frac{1}{3\,(-n)}$$

When the force N is applied directly at the edge, that is when $c=a$, the pressure at this point becomes infinite.

The above constitutes the investigation of Mons. Debauve. Before offering any comments upon it, I will add some observations which he makes upon the resistance to crushing of stone, so excellent and practical that they cannot but prove of interest to engineers.

After giving the usual tables of the strength of different varieties of stone, Mons. Debauve says:—

But too much confidence must not be placed in the above figures, for they take no account of a very important factor and one as yet but little studied, namely the influence of the *size* of the specimens tested upon their resistance.

Dupuit in his treatise upon bridges, gives the following results, based, it is true, more upon reasoning than actual experiment:

It has been found, he says, that with equal surfaces, the resistance increases as the section approaches a circle, and that it decreases with the height, particularly if the specimen be divided into several layers. We can easily understand that when several prisms bear upon one another, the pressure is unlikely to be transmitted uniformly over the whole surface. It may happen, therefore, that some parts will be strained beyond their resistance before a pressure is reached

which, if uniformly distributed, could have been safely sustained.

While upon this subject we will remark, that so far experiments have only been made with uniform pressures extending over the whole surface of the prisms tested. In actual practice however, it often happens that only a portion of the surface sustains pressure. In such cases, can we consider the surface pressed as being isolated, and as liable to be crushed by a pressure that would crush it if it were isolated? We do not think so, although it is customary to so consider it. As we have already stated, crushing only takes place as an effect of lateral dilatation, and when there exists an obstacle to such dilatation, the resistance to crushing is thereby increased. This is a principle well understood in practice. Thus, we ring the head of piles to prevent their splitting, and put iron bands around certain supports destined to bear heavy loads. Let us suppose for instance that a cube of stone five centimetres high crushes under a load of 10,000 kilos, or 400 kilos per square centimetre. We certainly should not conclude from that fact that crushing would ensue if we placed an iron column with a base of 25 square centimetres, in the middle of a block of the same stone, a square metre in area, and loaded it with a weight of 10,000 kilos. It is quite certain that we should find the resistance enormously increased.

The proposition that, under all circumstances, whenever the pressure reaches the limit of resistance crushing ensues, leads to absurd consequences. Thus, if we should calculate the area of the point of a well sharpened pointing tool, we would conclude that it would pass through granite like a needle through a piece of cloth, and that stone could be cut with a razor. The figures we have given (referring to table) only relate to pressures covering the whole surface of the specimen tested. If the pressure is local and only bearing upon certain points, the resistance increases in proportion as these points are farther from the sides, but we know nothing of the manner in which the resistance acts under these circumstances, and for lack of data follow incorrect assumptions.

Instead of multiplying experiments upon all the known stones used in building, we should endeavor, first to ascertain how resistance varies with the dimensions of the specimens tested, the pressure being uniformly applied; and secondly, how it varies under local pressures, applied to different parts of the surface.

Crushing is due, as Dupuit remarks, to the lateral expansion of the stone, the compressed parts endeavoring to escape at the sides, where they find no impediment to so doing. When the pressure becomes so great as to destroy the cohesion of the fibers, they burst asunder, and crushing ensues.

Any cause which opposes this lateral expansion will have for effect the retardation of the crushing of the material, by annulling more or less of the tendency to spread. Mons. Pelletreau in a recent paper corroborates the above statement by demonstrating the following proposition:

*Other things being equal, a prism of stone resists crushing in proportion to the degree of friction existing between its base and the surface upon which it stands.*

In another part of his volume, Mons. Debauve gives instances of very heavy pressures sustained by the pillars of some noted buildings. Thus:

| | lbs. per sq. inch |
|---|---|
| The pillars of the Invalides | 213 |
| The pillars of St. Peter's at Rome | 230 |
| The pillars of St. Paul's, London | 272 |
| The pillars of dome of the Pantheon. | 414 |
| The pillars of tower of St. Méry | 414 |
| The pillars of Church of Toussaint at Angers | 627 |

These are certainly enormous charges, much greater some of them, than any builder would knowingly incur at the present day. No doubt the architects of these edifices never gave the question of crushing a thought, but simply carried up their structures according to artistic design, and let the pressures be what they would. Mons. Debauve does not state whether the above pressures occur at the bottom of the foundations, under ground, or in free air. This would unquestionably make a great difference. I do not know what the elementary pressure upon the piers of the Brooklyn bridge is, but it must be something enormous.

It should be remembered too, that the sub-foundations of this structure consist of a heavy layer of *timber*, resting upon concrete, so the whole of the great weight rests on wood instead of stone. No doubt when the substructure is carried down to

such a great distance below the surface, the strong hold which the encasing material takes of its sides very materially lessens the weight actually resting on the bottom. Unquestionably pressures may be admitted below the surface, which would be dangerous if sustained by portions of the structure deprived of lateral support; besides it is certain that at great depths below the surface, the nominal pressures do not really exist, the weight being largely sustained by the adherence on the sides. For this reason, the sides of buried foundations should be built vertical, and not spreading out below ground.

Let us now return to Debauve's examination of Bresse's formulæ.

It would no doubt be unwise to attempt a thorough investigation of the problem without a careful study of Mons. Bresse's own solution of the given problem by the method of centers of percussion. I have not been able to find this latter author's work in the Astor library, and am therefore ignorant of his process of demonstration, but, as stated at the commencement of this article, his results have been generally accepted, and pass current among the most eminent European authorities without challenge. This is saying a great deal.

As regards Debauve's demonstration, if we admit his assumptions, the results follow naturally. Let us see what these assumptions are. He assumes (as does Bresse) a homogeneous mass. That is a mass, all the parts of which are uniformly bound together, so that any local pressure is disseminated throughout the mass. Thus, a local pressure acting vertically downward, does not bear wholly upon that portion of the base directly under it, but is transmitted to the other parts of the base as well. There can be no difficulty in accepting this assumption, for it is not a scientific abstraction, but a practical essential of good workmanship.

His next assumption is, that the base, while undergoing compression, is not distorted, but remains in a straight line. With this, we must also couple the tacit assumption that the mass rests upon an unyielding base, or one which is practically unyielding. As the structure, in the case of high dams, must stand on bed rock, we cannot refuse to admit this. Even if we consider the pressure in the upper courses of the work, we should still find, I think, that the upward reaction of the rigid base was operative through the intervening material.

The rigidity of the base being admitted, we seem forced to grant the absence of distortion. The only assumption which still remains to be examined is, that compression is proportional to pressure. This can be readily conceded, and it would then appear that Mons. Debauve has rested his demonstration upon reasonable grounds.

It may be remarked that in the problem announced, the sides of the assumed mass are supposed to be vertical. It is difficult to see how this condition affects the results, and it is neglected by Debauve in the application of the formulæ to dams.

It will be observed that he introduces a distance, represented by $x$, (Fig. 2), of which no use is made in the demonstration.

One word may perhaps be usefully added upon the subject of compression. All building materials are more or less compressible, and all buildings undergo more or less compression from their own weight and from such exterior forces as they may be subjected to. In the case of masonry this compression takes place mostly in the mortar. But a point is sooner or later reached, when compression ceases, or else the whole structure would flatten out into a sheet of infinite thinness. The mass has then become incompressible. If the pressure still augments, another action takes place, namely, that of crushing. We may say that there are three distinct stages in the phenomena accompanying pressure applied to stonework. First, there is the compression due to the artificial and composite character of the mass considered as a whole. This consists in taking up the vacancies of the work, or in the settling of the structure to its bearings. Then comes the condensation, due to the natural compressibility of the materials when brought in actual contact with each other. This is no doubt slight when compared to the former contraction. The pressure still augmenting destruction of the cohesion of the material occurs; that is crushing and disentegration. These three actions may be going on simultaneously in different parts of the same structure; and we may have, according to circumstances, either the establishment of equilibrium, or destruction of the edifice.

# BICYCLES AND TRICYCLES.*

By C. V. BOYS.

From the "Journal of the Society of Arts."

THE subject of this paper is one of such wide interest, and of such great importance, that it is quite unnecessary for me to make any apology for bringing it before your notice. Exactly two months ago, I had the honor of dealing with the same subject at the Royal Institution. On that occasion I considered main principles only, and avoided anything in which none but riders were likely to take an interest, or which was in any way a matter of dispute. As it may be assumed that the audience here consists largely of riders, and of those who are following those matters of detail, the elaboration, simplification, and perfection of which have brought the art of constructing cycles to its present state of perfection, I purpose treating the subject from a totally different point of view. I do not intend, in general, to describe anything, assuming that the audience is familiar with the construction of the leading types of machines, but rather to consider the pros and cons of the various methods by which manufacturers have striven to attain perfection, As a discussion on the subject of this paper will doubtless follow—and I hope makers or riders of every class of machine will freely express their opinion, for by so doing they will lend an interest which I alone could not hope to awaken—I shall not consider it necessary to assume an absolutely neutral position, which might be expected of me if there were no discussion, but shall explain my own views without reserve.

The great variety of cycles may be grouped under the following heads:—

1. The Bicycle unmodified.
2. The Safety bicycle, a modification of 1.
3. The Center-cycle.
4. The Tricycle, which includes five general types:—
   (*a.*) Rear steerer of any sort.
   (*b.*) Coventry rotary.
   (*c.*) Front steerer of any sort (except *e*).
   (*d.*) Humber pattern.
   (*e.*) The Oarsman.
5. Double machines: sociables and tandems.
6. The Otto.

It is perfectly obvious that not one machine is superior to all others in every respect, for if that were the case, the rest would rapidly become extinct. Not one shows any signs of becoming extinct, and, therefore, it may be assumed that each one possesses some points in which it is superior to others, the value of which is considered by its riders to far outweigh any points in which it may be inferior. The widely varying conditions under which, and purposes for which, machines are used, and the very different degrees of importance which differently constituted minds attach to the peculiarities of various machines will, probably, prevent any from becoming extinct. Nevertheless, the very great advantages which some of these possess over others will, no doubt, in time become evident by the preponderance of the better class of machines.

The bicycle, which surpasses all other machines in simplicity, lightness, and speed, will probably, for these reasons, always remain a favorite with a large class. The fact that it requires only one tract places it at a great advantage with respect to other machines, for it is common for a road which is unpleasant from mud or stones to have a hard smooth edge, a kind of path, where the bicyclist can travel in peace, but which is of little advantage to other machines. Again, the bicycle can be wheeled through narrow gates or doorways, and so kept in places which are inaccessible to tricycles. One peculiarity of the bicycle, and to a certain extent of the center-cycle, is that the plane of the machine always lies in the direction of the resultant force, that the machine leans over to an amount depending on the velocity and the sharpness of the curve described. For this reason all lateral strain on the parts is abolished, and if we except the slipping away of the

* A paper read before the Society of Arts.

wheel from under the rider, which can hardly occur on a country road, an upset from taking a curve too quickly is impossible. This leaning to either side by the machine and rider gives rise to that delightful gliding which none but the bicyclist or the skater can experience. In this respect the bicycle has an enormous advantage over any machine, tricycle or Otto, which must at all times remain upright, and which must, therefore, at a high speed be taken round a curve with discretion.

The perfect and instantaneous steering of the bicycle, combined with its narrowness, counteract, to a great extent, the advantage which the tricyclist has of being able to stop so much more quickly, for the bicyclist can "dodge" past a thing for which the rider of the three wheeler must pull up. In one other respect the bicyclist has an advantage, which, though of no real importance, has great weight with many people. The bicycle well ridden presents a picture of such perfect elegance that no one on anything else need expect to appear to advantage in comparison.

The chief disadvantage of the bicycle is the fact that a rider cannot stop for any purpose, or go back a little, without dismounting. For town riding, where a stoppage is frequently necessitated by the traffic, this perpetual mounting and dismounting is not only tiresome but wearying, so much so, that few bicyclists care to ride daily in town.

The position of the rider on a bicycle with respect to the treadles is by no means good, for if he is placed sufficiently forward to be able to employ his weight to advantage without bending himself double, he will be in so critical a position that a mere touch will send him over the handles. He has, therefore, to balance stability and safety against comfort and power; the more forward he is, the more furiously he can drive his machine, and the less does he suffer from friction and the shaking of the little wheel: the more backward he is, the less is he likely to come to grief riding down hill, or over unseen stones. The bicyclist is no better off than the rider of any other machine with a little wheel, the vibration from which may weary him nearly as much as the work he does. The little wheel as a mud-throwing engine is still more effective on the bicycle than it is on any tricycle, for in general it is run at a higher speed.

I now come to the usual complaint about the bicycle. There is a fashion just now to call it dangerous, and the tricycle safe. But the difference in safety has been much exaggerated. The bicyclist is more likely to suffer from striking a stone than his friend on three wheels, but then he should not strike one where the tricyclist would strike a dozen. Properly ridden, neither class of machine can be considered dangerous; an accident should never happen except it be due to the action of others. People, carts, cattle, and dogs on the road are liable to such unexpected movements, that the real danger of the cyclist comes from the outside; to danger from absolute collapse due to a hidden flaw in the materials employed, every one is liable, but the bicyclist more remotely than the tricyclist, owing to the greater simplicity of his machine. The bicyclist, though he has farther to fall in case of an accident from any of these causes, is in a better position than the tricyclist, for he is outside instead of inside of his machine; he can in an instant get clear. It would appear that many tricyclists consider accidents of the kind next to impossible, for in several machines the rider is so involved, that an instantaneous dismount without a moment's notice, at any speed, is absolutely impossible. There remains one objection, which, however, should be of next to no importance—the difficulty of learning the bicycle prevents many from taking to the light and fast machine, because they are afraid of a little preliminary trouble.

The chief objections to the bicycle, then, are the liability of the rider to go over the handles, the impossibility of stopping very quickly, and the inability to remain at rest or go backwards, and the difficulty of learning.

The first two of these are, to a large extent, overcome in the safety bicycles, but not without the introduction of what is in comparison a certain degree of complication, or without the loss of the whole of the grace or elegance of the bicycle. On almost all these safety bicycles the rider is better placed than on the unmodified bicycle, but though safer I do not think bicyclists find them compete in

speed, though, no doubt, they are superior in that respect to the tricycle. Though they do not allow the rider to stop without dismounting, the fatigue resulting from this cause is less than it is with a bicycle,owing to the fact that with the small machines the rider has so small a distance to climb. Of these machines, the "Extraordinary" leaves the rider high up in the air on a full-sized wheel, but places him further back and more over the pedals. The motion of these is peculiar, being not circular but oval, a form which has certain advantages.

In the Sun and Planet and Kangaroo bicycles, a small wheel is "geared up," that is, is made to turn faster than the pedals, so as to avoid the very rapid pedaling which is necessary to obtain an ordinary amount of speed out of a small wheel. In each of these the pedals move in a circular path, and their appearance is in consequence less peculiar than that of the Facile, which, in this respect, does not compare favorably with any good machine. The pedal motion on the Facile is merely reciprocating. Riders of machines where circular motion is employed, among them myself, do not believe that this reciprocating motion can be so good as circular, but I understand this view is not held by those who are used to it Of course, the harmonic motion of the Facile pedal is superior to the equable reciprocating motion employed in some machines where speed is an object, especially with small wheels.

If I have overlooked anything typical in the modified bicycle class, I hope some one will afterwards supply the omission, and point out any peculiarities or advantages.

That very peculiar machine, the Centercycle seems to combine many of the advantages of the bicycle and tricycle. On it the rider can remain at rest, or can move backwards; he can travel at any speed around curves without an upset being possible; he can ride over brickbats, or obstructions, not only without being upset, but if going slowly, without even touching them. As this machine is very little known, a few words of explanation may be interesting.

In the first place, the rider is placed over the main wheel, as in the bicycle,but much farther forward. There are around him, on or near the ground, four little wheels, two before and two behind, supported in a manner the ingenuity of which calls for the utmost admiration. Turning the steering handle not only causes the front and rear pairs to turn opposite ways, but owing to their swivelling about an inward pointing axis, the machine is compelled to lean over towards the inside of the curve: not only is this the case,but each pair rises and falls with every inequality of the road, if the rider chooses that they run on the ground; but he can, if he pleases, arrange that in general they ride in the air, any one touching at such times as are necessary to keep him on the top of the one wheel on which alone he is practically riding. He can, if he likes, at any time, lift the main-weeel off the ground, and run along on the others only. The very few machines of the kind which I have seen have been provided with foot straps, to enable the rider to pull as well as push, which is a great advantage when climbing a hill, but this is on every machine except the Otto, of which I shall speak later, considered a dangerous practice.

Some of the objections to the bicycle to which I have referred were sufficient to prevent many, especially elderly men, from dreaming of becoming cyclists. So long as the tricycle was a crude and clumsy machine, there was no chance of cycling becoming a part, as it almost is, and certainly soon will be, of our national life. The tricycle has been brought to such a state of perfection, that it is difficult to imagine where further progress can be made.

Perhaps it will be well to mention what is necessary, in order that a three-wheeled machine may be made to roll freely in a straight line, and also round curves. At all times each wheel must be able to travel in its own plane in spite of the united action of the other two. To run straight, the axes of all the wheels must obviously be parallel. To run round a curve, the axis of each must, if continued, pass through the centre of curvature of the curve. If two wheels have a common axis, the intersection of the two lines forming the axis can only meet in one point. To steer such a combination, therefore, the plane of the third wheel only need be turned. If the axes of no two are common, then the planes of two of the wheels must be turned in

order that the three axes may meet in a point.

Not only does free rolling depend on the suitable direction of the planes of the wheels, each wheel must be able to run at a speed proportional to its distance from the point of intersection of the three axes, *i. e.*, from the ever-shifting center of curvature.

The most obvious way, then, of contriving a three-wheeler is to drive one wheel, steer with another, and leave the third, which must be opposite the driver, idle. The next in simplicity is to drive with one wheel, and steer with the other two, having one in front, and the other behind. So far, then, the single driving rear-steerer and the Coventry rotary pattern are easily understood. The evils of single driving minimised, it is true, to a large extent in the Coventry rotary, have led to the contrivance of means by which a wheel on each side may be driven without interfering with their differential motion in turning a corner. Three methods are commonly used, but as only two are employed on tricycles, I shall leave the third till I come to the special machine for which it is necessary. The most easy to understand is the clutch, a model of which I have on the table. If each main wheel is driven by means of one of these, though compelled to go forward by the crankshaft, it is yet free to go faster without restraint. By this means "double driving" is effected in several forms of tricycle. Differential gear, which is well understood, and of which there are several mechanically-equivalent forms, divides the applied driving power, whether forwards or backwards, between the main wheels, equally if the gear is perfect, unequally if imperfect. To understand the effect of the two systems of driving, and of single driving, let us place on grooves a block which offers resistance to a moving force. If we wish to move it, and apply our force at the end of one side, it will tend to turn round as well as move forward, and much friction will be spent on the guides by their keeping it straight. This is the single driver. If, instead of applying force at one side, we push the block bodily forward by a beam moving parallel to itself, then so long as the guides are straight no strain will be put upon them, even though one side of the block is resisted more than the other; if, however, the guides compelled the block to travel round a curve, then the power, instead of being divided between the two sides in such proportion as is necessary to relieve the guides of all strain, is suddenly applied only to the inside, and the effect is that of a single driver only. This is the clutch. Lastly, if the last-mentioned beam, instead of being pushed along parallel to itself, were pivoted in the middle, and that pivot only pushed, the same power would be applied to each side of the block, and no strain would be thrown on the guides, whether straight or curved, so long as the resistance opposed to the block on the two sides were equal; if, however, one side met with more resistance than the other, then the guides would have to keep the block straight. This is the differential gear. I have assumed that in the last case the force was applied to the middle of the beam; this corresponds to every evenly-balanced gear. In the gear employed by Singer, which is not evenly balanced, but which derives its good qualities from its simplicity, the same effect is produced as if the beam were pivoted on one side of the center, instead of on the center. Thus, though both sides are driven, one is driven more than the other. On the whole, there is no doubt that the balanced gear gives a superior action to the clutch, for except when the two sides of the machine meet with very different resistance, and then only when running straight, the clutch will not compare with the other. The clutch also gives rise to what is considered by most riders a grave defect, the inability to back treadle, while the free pedal, which is an immediate consequence, is considered by others a luxury. On the other hand, this same free pedal can be obtained on differentially-driven machines to which speed and power gear have been applied.

Of the relative merits of different forms of differential gear there is little to be said. Perhaps it will not be thought I am unduly thrusting myself forward, if I refer to a scheme of my own, in which no toothed wheels are employed, but in which two conical surfaces are driven by a series of balls lying in the groove between them, and jambed against them by a recessed ring. I have here a large wooden diagrammatic model, and a small working model in steel, which shows that

the new principle employed is correct, namely, that a ball while jambed is free to turn, or if turning is able to jamb. All Humbers, and most front steerers, employ differential gearing; in some front steerers the clutch of necessity is used.

Neglecting for the present the different modes of transmitting power from the pedals to the main wheels, it is possible now to consider the four typical builds of tricycle. The only advantage that a rider can find in a rear-steerer is the open front, so that in case of accident he can more easily clear himself of his machine; as I have already remarked, this power of instantly escaping seems to be considered by many as of no importance. In a rear-steerer which has not an open front, whether driven by a clutch or by differential gear, I fail to discover any good quality. The steering of a rear-steerer is so very uncertain, that such machines cannot safely be driven at anything like a high speed, because any wheel meeting with an obstruction will, by checking the machine, diminish the weight on the steering wheel just at the time when a greater weight than usual should be applied. It is for the corresponding reason that the steering of a front-steerer is so excellent; the more the machine is checked by obstruction, by back treading or by the brake, the greater is the weight on the front wheel. For shooting hills, or for pulling up suddenly, no machine of any kind will compare with a good front-steerer. In all respects it is superior to the rear steerer, if we except the open front, but against this may be set the fact that on many the rider can mount from behind, or can dismount in the same manner while the machine is in motion. Experience shows that the front-steerer is, for general excellence, safety, easy management, and light running, the best all-round tricycle that is to be had.

The Humber build, which departs less from the ordinary bicycle than any other, is far superior to all others for speed; it is, however, somewhat difficult to manage, for the steering is not only delicate, but critical, requiring constant care lest a stone or other obstruction should take the rider unawares, and steer the machine for him. The control which a skillful rider of the Humber has over his machine is wonderful; the elegance of the machine among tricycles is unequaled. So great a favorite is this form, especially among the better class of riders, that almost every firm have brought out their own Humber, each with a distinguishing name. The only improvement or change, whichever it may be, that has been made by others with which I am acquainted, is the triple steering, in which the hind wheel moves the opposite way to the others. The corresponding change in the bicycle was soon discarded; I do not know what advantage can result from the increased delicacy of steering here; I should have thought it delicate enough already.

One noticeable change in the front-steering tricycle, which has been largely made lately, is the substitution of central for side-gearing, in consequence of which bicycle cranks can be employed, instead of the cranked axle, with its fixed throw. This gives an appearance of lightness which the older types of machine do not possess.

I now come to that very difficult and all-important subject, the method of transmitting power from the body of the rider to the main axle. Next to the structural arrangement, this is most important in distinguishing one type of machine from another.

The first to which I shall refer is the direct action employed on the "National" and the "Monarch" tricycles. It is obvious that by having no separate crank shaft, much greater simplicity and cheapness, and less friction are attained, than can be possible when the extra bearings and gear generally used are employed. In this respect the direct action machines undoubtedly have an advantage, but an advantage of any kind may be too dearly bought, as it certainly is here. In the first place, the direct action can only be applied to a rear-steering, clutch-driven machine, or single driver, for if the wheels were not free to run ahead, it would be impossible to go round a curve. In the second place, the rider must be placed at such a height for his feet to work on the axle, that the machine of necessity is very unstable, and is likely to upset if ridden without great caution round a curve. Thirdly, to diminish as far as possible this last objection, miserable little wheels must be employed, which cannot be geared up, that is, made to travel faster than the treadles, and so be

equivalent to larger wheels. Therefore, though it is likely that at such low speeds only as it is safe to run such a machine, it may move more easily than a machine of a recognized type, and though direct action would undoubtedly be advantageous if it did not entail defects of a most serious order of magnitude, we may dismiss this at once from our consideration. It is true that in the "Monarch" a few inches of height are gained by the hanging pedals, but I question very much whether one machine is much better than the other.

The chain which is used on almost every make of machine cannot be considered perfect; it is, on the whole, a dirty and noisy contrivance, giving rise to friction where the links take and leave the teeth of the pullies; stretching, or rather lengthening, by wear, and, finally, allowing back lash, which is most unpleasant. In spite of all this it affords a convenient and reliable means of transmitting power, which is applicable to every type of tricycle except one.

Instead of a chain, an intermediate or idle wheel has been tried, but this has not been found advantageous. The intermediate wheel has been removed, and the crank and wheel pulley allowed to gear directly together, making reverse motion of the feet necessary, and possibly reducing friction.

The crank and connecting rod are employed in some machines. If there are two only, they must not be placed in opposite positions, but be fixed at an angle, so that there are times when each rod is under compression, a strain which delicate rods cannot stand. In the three-throw crank, employed in the matchless tricycle, this objection is obviated, for one, at least, is at all times in such a position as to be in tension. The objection to the crank is the fact that it weakens the shaft, and that it can only be used with a clutch, not with a differential gear.

The most silent, neatest, and cleanest driver, the one of which the working friction is least, is the endless steel band, so well known in connection with the Otto bicycle. This is not, as far as I am aware, employed on any tricycle, makers probably fearing lest it should slip. The Otto shows that it can safely be employed.

I have devised a scheme, of which I now show a model, which seems to me to be free from the objections which may be urged against other methods; but I, of course, cannot be considered in this respect a judge. Eccentrics are well known as equivalent to cranks, but if used in the same way, with a connecting rod, either fatal friction or enormous ball-bearings would be necessary. Instead of these, I connect two pair of equal eccentrics by an endless band embracing each, so that the band acts like a connecting rod without friction, and at the same time acts by its turning power as on the Otto, thus making two eccentrics sufficient instead of three, and carrying them over the dead points.

There is one more system of transmitting power employed on a few machines. In these a band or line passes over the circumference of a sector or wheel, and the power is directly applied to it. The motion of the feet in the omnicycle, and of the hands and body in the oarsman, is therefore uniform. There would be no harm in this if it were not for the starting and the stopping, which cannot be gradual and at the same time effective in machines of this type. For this reason a high speed cannot be obtained; nevertheless, these machines are better able to climb hills than are tricycles with the usual rotary motion, for, at all parts of the stroke—which may be of any length that the rider chooses—his driving power on the wheels is equal. The ingenious expanding drums on the omnicycle makes this machine exceptionally good in this respect, for increased leverage is effected without increased friction, which is the result of "putting on the power" in some of the two-speed contrivances.

Having spoken of the oarsman tricycle, I must express regret that I have not been able to find an opportunity to ride on or with the machine, so that I cannot, from observation, form an opinion of its going qualities. There can be no doubt that the enormous amount of work that can be got from the body in each stroke on a sliding seat in a boat must, applied in the same manner on the oarsman tricycle, make it shoot away in a surprising manner; whether such motion, when continued for hours, is more tiring than the ordinary leg motion only, I cannot say for certain, but I should imagine that it would be. The method by which the

steering is effected by the feet, and can with one foot be locked to a rigidly straight course, is especially to be admired.

There is much difference of opinion with respect to the most suitable size for the wheels of machines. Except with certain machines, this has nothing to do with the speed at which the machine will travel at a given rate of pedalling, for the wheels may be geared up or down to any extent that is made to turn more quickly or slowly than the cranks. This, the most suitable speeding, is a separate question, and must be treated by itself.

Large wheels are far superior to small wheels in allowing comfortable easy motion, a matter of considerable importance in a long journey. They are also far better than small for running over loose or muddy ground, for, with a given weight upon them, they sink in less, from the longer bearing they present, and this, combined with their less curvature, makes the everlasting ascent which the mud presents to them far less than with a smaller wheel. On the other hand, the large wheel is heavier, and suffers more from air resistance than the small wheel. For racing purposes a little wheel, geared up of course, is certainly better than a high wheel; for comfortable traveling, and in general, the high wheel is preferable. Though this is certainly the case, it does not follow that large wheels are worth having on a machine when there is already one little wheel. If the rider is to be worried with the evils of a little wheel at all, it is possible that any advantage which large wheels would give him would be swamped by the vibration and mud sticking properties of the small steering wheel. One firm, in their endeavors to minimize these evils, have designed machines without any very small wheels; all three wheels are large, and a steadier and more comfortable motion no doubt results.

High and low gearing are the natural sequel to high and low wheels. Of course, the lower the gearing the greater is the mechanical advantage in favor of the rider when meeting with much resistance, whether from wind, mud, or steepness of slope. In spite of this, for some reason which I cannot divine, the machines with excessively low gear do not seem to obtain so great an advantage in climbing hills as might be expected. To make such a machine travel at a moderate speed only, excessively rapid pedalling is necessary, and the rider is made tired more by the motion of his legs than by any work he is doing. The slow, steady stroke by which a rider propels a high-geared machine is far more graceful and less wearying than the furious motion which is necessary on a low-geared machine. The height up to which the driving wheels are usually geared may be taken as an indication of the ease with which any class of machines runs. A rider on a low-geared machine can start his machine much more quickly than an equal man on one that has high gearing, and therefore in a race he has an advantage at first, which he speedily loses as his rapid pedalling begins to tell. For ordinary riding, the slight loss of time at starting is a matter of no importance whatever.

There are several devices which enable us to obtain the advantages of high and low gearing on the same machine, which at the same time give the rider the benefit of a free pedal whenever he wishes. On some single driving rear-steering tricycles the connection on one side is for speed, and that on the other for power, either being in action at the wish of the rider, or both speed and power combinations are applied on the same side. To drive with a power gear a single wheel only seems to me the height of folly; in my opinion no arrangement of this type is worthy of serious attention. Among the better class of machines there are three methods by which this change is effected—First, that employed on the omnicycle, to which I have already referred; secondly, an epicyclic combination of wheelwork which moves as one piece when set for speed, thus adding nothing to the working friction except by its weight, but which works internally when set for power, thus reducing to a small extent, by the additional friction, the gain of power which the rider desires; thirdly, a double set of chains and pulleys, each set always in movement, so that whether set for speed or power, there is rather more friction than there would be if there were no additional chains, but these are free from that increased friction due to toothed-wheel gearing, from which the epicyclic contri-

vances suffer only when set for power. There is much difference of opinion whether any of these arrangements are worth carrying, for perhaps nine miles, for the sake of any advantage that may be obtained in the tenth. It is on this account that the drums on the omnicycle are so excellent; whether expanded or not, there is, on their account, no loss of work whatever, for there is no additional friction. The subject of these two speed gears will, I hope, be discussed; it is one which, though not new, is coming more to the front, and about which much may be said.

Having now dealt with the means by which tricycles are made to climb hills more easily, I wish to leave the subject of bicycles and tricycles altogether for a few minutes, to say a few words which may specially interest those who are fond of trying their power in riding up our best known hills. The difficulty of getting up depends to a large extent on the surface and on the wind, but chiefly on the steepness. The vague manner in which one hill is compared with another, and the wild ideas that many hold who have not made any measurements, induces me to describe a method which I have found specially applicable for the measurement of the steepness of any hill on which a cyclist may find himself, and also a scheme for the complete representation of the steepness and elevation of every part of a hill on a map so as to be taken in at a glance. The force required to move the thing up a slope is directly proportional, not to the angle, but to the trigonometrical sine of that angle. To measure this, place the tricycle, or Otto—a bicycle will not stand square to the road, and therefore cannot be used—pointing in direction at right angles to the slope of the hill, so that it will not tend to move. Clip on the top of the wheel a level, and mark that part of the road which is in the line of sight. Take a string made up of pieces alternately black and white, each exactly as long as the wheel is high, and stretch it between the mark and the top of the wheel. If there are $n$ pieces of string included, the slope is 1 in $n$, for by similar triangles the diameter of the wheel is to the length of the string as the vertical rise is to the distance on the road. This gives the average steepness of a piece sufficiently long to be worth testing, because an incline, only a few feet in length, of almost any steepness can be mounted by the aid of momentum.

There is only one process with which I am acquainted which supplies a method of representing on a map the steepness of a road at every part. Contours, of coures, show how far one has to go to rise 50 or 100 feet, but as to whether the ascent is made uniformly or in an irregular manner with steep and level places, they tell us nothing. Let the course of a road be indicated by a single line where it is level, and by a pair of lines where inclined. Let the distance between the lines be everywhere proportional to the steepness, then the greatest width will show places of intermediate steepness, the crossing of the lines, which must be distinguishable from one another, will show where the direction of the slope changes. Further, the size of the figure bounded by the two lines will show the total rise; a great height being reached only by great steepness or by great length, a large figure being formed only by great width or by great length. Those who are mathematically inclined will recognize here that I have differentiated the curve representing the slope of the hill, and laid the differential curve down in plan.

Having wandered off my subject, I must return to more mechanical things, and give the results of some experiments which I have made on the balls of ball bearings. There is no necessity to argue the case of ball *v.* plain bearings, the balls have so clearly won their case, that it would be waste of time to show why. Of the wear of the twelve balls forming one set belonging to the bearings of the wheels of my Otto, I have on a previous occasion spoken; I may, however, repeat that in running 1,000 miles, the twelve balls lost in weight only $\frac{1}{20.8}$ grain, or each ball lost only $\frac{1}{240}$ grain. The wear of the surface amounted to only $\frac{1}{1350000}$ inch; at the same rate of wear, the loss in traveling from here to the moon would amount to only $\frac{1}{34.3}$ of their weight. I examined each ball every 200 miles, and was surprised to find that on the whole the wear of each, during each journey

varied very little. The balls experimented on were a new set obtained from Mr. Bown. I also had from him one ball of each of the following sizes 3, 4, 5, 6, and 7–16ths of an inch in diameter, as I was curious to know what weight they would support without crushing. As a preliminary experiment, I placed a spare $\frac{5}{16}$ ball between the crushing faces of the new testing machine at South Kensington, and applied a gradually increasing force up to 7 tons 9½ cwt., at which it showed no signs of distress. On removing it I found that it had buried itself over an angle of about 60° in the hard faces, faces so hard that a file would not touch them. Those marks will be a permanent record of the stuff of which the ball was made. The ball itself is sealed in a tube, so that any one who is curious to see it can do so. Finding that the crushing faces were not sufficiently hard, I made two anvils of the best tool steel, and very carefully hardened them. These, though they were impressed slightly, were sufficiently good for the purpose. In the following Table are the results of the crushing experiments:—

$\frac{3}{16}$ ball at 2 tons 13 cwt. did not break, but crushed on removing part of the weight.

¼ ball at 3 tons 15 cwt. did not break, but crushed on removing part of the weight.

$\frac{5}{16}$ ball at 4 tons 9 cwt. broke.

⅜ ball at 8 tons 6 cwt. did not break, crushed under another 120 pounds.

$\frac{7}{16}$ ball crushed before 3 tons, with which I was starting, had been applied. Examination showed that the steel bar of which it was made had been laminated.

These experiments do not tell much of importance; they are curious, and, perhaps, of sufficient interest to bring before your notice. The fragments are all preserved in tubes, and labeled so that anyone who likes to see them can do so.

Of the advantage which a machine which will collapse or fold up when desired, but retain its form on the road, offers in convenience, it is unnecessary for me to speak.

Of double machines, the Rucker tandem bicycle seems to me to be in every respect the best, but I should add that I speak only from imagination and not from experience. The independent steering, the impossibility of capsizing forwards or sideways, the position of the rider over his work, the absence of any little wheel with its mud-throwing and vibrating tendencies, combine to make a machine which ought to be superior in almost every desirable quality to any other; what it may be in practice I hope to hear in the discussion.

Of double tricycles, the Sociable has been tried by many, and is practically a failure in so far as traveling quickly and easily is concerned. The Tandem, though it presents so objectionable an appearance, seems likely to become a favorite, for it surpasses any single tricycle, and rivals the bicycle in speed. How it may compare in comfort or in safety with the single machine, perhaps those few who are well acquainted with them will say; at any rate, in the case of the Humber, greater stability is given to the steering owing to the weight of the front rider.

Time will not allow me to say more of these machines, or to attack the subject of steam, electric, or magic tricycles, which I had hoped to do. With steam and electricity we are well acquainted; by magic tricycles, I mean those driven by a motor which, without any expense, will drive one twenty miles an hour, up or down hill, with perfect safety. Highway regulations, and certain reasons not well understood, have at present prevented these contrivances from making a revolution.

There remains one machine which must be considered separately, for it cannot be classed with any other. This is the Otto bicycle. My opinion of this machine is so pronounced that I do not care to state it fully. I shall merely give the reasons why I prefer it to anything else, and in so doing I shall be taking the first step in the discussion, in which it will be interesting to hear from riders of other machines the reasons for their preference.

In the first place, the evils of a third or little wheel, the cause of trouble in all tricycles, are avoided. There is none of the vibration which makes all other machines almost unbearable to Ottoists, vibration which tricyclists have learned to consider a necessary accompaniment of cycling, but which has, no doubt, been diminished by the use of the spring support of the front-steering Humber. It would be presumptuous in me to make any remarks on the effect of this vibration on the human system; we shall all be anxious to hear what our Chairman

has to say on this point. By having only two wheels we have only two tracks, so that we can travel at a fair speed along those places in the country called roads, which consist of alternate lines of ruts and stones, where a three-track machine could not be driven, and where, from the quantity of loose limestone in the ruts, a little wheel of a two-track tricycle would be likely to suffer. By having no little wheel, we can ride in dirty weather without having the rest of our machine pelted with mud, so that cleaning takes less time than it does with anything else. As I have already remarked, the small wheel is the culprit which makes the bicycle and tricycle drive so heavily on a soft road. The ease with which the Otto can therefore be run through the mud, astonishes everyone. Having no little wheel, we can obtain the full advantage of the high 56 inch wheel, which almost everyone prefers. As I have ridden all combinations, from a 50-inch geared up to 60-inch, to a 60-inch geared level, I can speak from experience of the increased comfort to be derived from these large wheels, though for speed only they do not compare with the smaller and lighter wheels geared up. A further point gained by the use of two wheels only is the fact that the whole weight of machine and rider is on the driving-wheel, as it is also on the steering-wheels, so that by no possibility can the wheels be made to slip in the driving, or to fail in steering from want of pressure upon them.

The most important consequence, however, is the absence of any fixed frame. In all machines, bicycles and tricycles, with the usual fixed frame, a position is found for the saddle, which is, on the whole, most suitable. For some particular gradient it will be perfect, on a steeper gradient the treadles will be further in advance, but with a steeper gradient the rider should be more over the front of the treadles. To get his weight further to the front, he has to double up in the middle, and assume a position in which he cannot possibly work to advantage. The swinging frame of the Otto carries the treadles, of necessity, further back, so that the Ottoist when working at his hardest, is still upright, with his hands in the line between his shoulders, and his feet and his arms straight, so that he can hold himself down, and employ his strength in a perfectly natural position. On going down a slope, the fixed frame of a bicycle or tricycle leans forward, and places the rider in such a position that extra weight is thrown on his arms and shoulders, whereas the swing frame of the Otto goes back, and the rider of necessity assumes that position in which his arms are relieved of all strain. In so far as the general position taken by the automatic Otto frame is concerned, nearly the same effect can be obtained by using the swing frame of the Devon tricycle, which can be shifted and locked in any position which the rider wishes, or by the sliding saddle which can be slid backwards or forwards and locked so as to place the rider in one of three positions. Though the rider can by these devices assume nearly that position with respect to the treadles which is most advantageous, he cannot obtain that curious fore and aft oscillation made use of by the Ottoist in climbing hills, which, as the model on the table shows, enables him to get past the dead points without even moving, and which, therefore, makes the Otto by far the best hill-climbing machine there is, if account is taken of the high speeding with which all Ottoists ride. This is a proposition which none who knows the machine will question for one moment.

The freedom of motion resulting from the swing of the frame of the Otto gives a pleasurable sensation, which those who have only experienced the constrained motion of a three-wheeler cannot even understand.

The very peculiar method of driving and steering which seems so puzzling to the novice, especially if he is a good rider of other machines—for in that case he is far worse off than one who has never ridden anything—give the rider, when he is familiar with them, a control over the machine which is still surprising to me. In the first place, the machine will run along straight, backward, or forwards, so long as the handles are let alone. This automatic straight running is a luxury, for till a deviation has to be made, the steering handles need not be touched, and the rider may, if sufficiently confident, travel with his arms folded or his hands in his pockets. The rigid connection between the cranks and the wheels does away with all the back-lash, which is so unpleasant

with chain or toothed-wheel gearing. There is no differential gear or clutch, but the machine possesses the advantage of the clutch over the differential gear when meeting with unequal resistance on a straight course, for each wheel must travel at the same speed; but, in turning a corner, instead of driving the inner wheel only, which is done by the clutch or both wheels equally, which is the case with differential gear, each wheel is driven, but the outer one more than the inner. At high speeds, the steering of the Otto has this advantage, that whereas, with a given action on a tricycle, the same deviation will be effected in the same *space* at high as at low speeds, the same action on the Otto will, at high speeds, produce the same deviation in the same *time* as it does at low speeds; and so instead of becoming more sensitive at high speeds, as is the case with the tricycle, the steering of the Otto remains the same. This is because the steering of the tricycle depends on a kinematical, that of the Otto on a dynamatical principle.

In another respect, no machine can approach the Otto; at almost any speed the rider can, if there is reason, instantly dismount, by which action he puts on the brakes, and the machine will save him from falling, stopping with him almost instantly. As is well known, we can move backwards and forwards, we can twist round and round in our own width, or can ride over bricks with impunity.

One objection to the machine is the difficulty of learning, which is considerable, but which presents no danger. This difficulty has been much exaggerated, for before the present powerful brake was applied, it did require considerable skill to ride it down a steep hill. The way to do this must still be learnt, but it is now comparatively easy. For going down steep hills, the front-steering tricycle is without a rival; I do not know what other machine will do this better than the Otto. Lastly, the foot straps, which would be a great advantage on any machine, if only they were safe, are not—though none but riders will believe it—in any way a source of danger on the Otto. Having ridden this machine for close upon 10,000 miles, I can speak with more authority on this point than can those who are not able to sit upon it for a moment.

The only disadvantage which the machine presents is the fact that it is impossible to remove the feet from the pedals while running without dismounting; but though they must at all times follow the pedals, the Ottoist is not, as is generally thought, working when descending a hill.

The enthusiastic terms in which every one who has mastered the peculiarities of the Otto speaks of it would be considered as evidence in its favor, if we were not all considered by other cyclists to be in various stages of lunacy.

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## THE SEWERAGE AND REFUSE-DISPOSAL OF TOWNS.

By Prof. VIRCHOW.

From Abstracts of the Institution of Civil Engineers.

At the tenth meeting of the German Health Society, the following six theses were submitted by Professor Virchow, of Berlin, with a view to their discussion, but not for formal adoption.

1. Water-cleansing and water-seals are essentially needed for soil-pipes in houses.

2. Long-continued storage of fecal matters, either in middens or cesspools, in boxes or in tubs, must be avoided.

3. Choice may be made, in accordance with local circumstances, either of the direct removal of the excreta from dwellings in tubs, or of their discharge into closed channels or drains.

4. The introduction of sewage into public watercourses is in all cases objectionable. In towns having upwards of 100,000 inhabitants, this practice should in no case be permitted. In towns under 100,000 it should only take place where the conditions of current-velocity are especially favorable, and then only when proper precautions are adopted for disinfection and defecation.

5. The introduction, also, into public

watercourses of street and house drainage water, in the case of large and moderate sized towns, should only be permitted after thorough deposition of the sediment, which process may require to be further aided by chemical treatment, in accordance with the quantity and quality of the water which has to be dealt with.

6. The agricultural employment of fecal matters must be subject to strict sanitary supervision; but no more should be expected from sewage grounds, in the vicinity of towns than what has been insisted upon in the case of ordinary agricultural operations of a like nature.

In dealing with the first proposition, Professor Virchow stated that even Captain Liernur, who had been foremost in asserting the sufficiency in his closets of a seal consisting of urine, had recently abandoned this position, and had admitted the advisability of the use of water. With respect to the second proposition, he pointed out the impossibility of ensuring the necessary sanitary conditions when any system, hitherto introduced, of storing excrement in the vicinity of dwellings was resorted to. Even in the case of tubs the intervals of removal were frequently so lengthy that dangerous decomposition might arise.

The three following theses are intimately connected, and Dr. Virchow alluded to the difficulty of removing polluting ingredients from water, and stated that he was greatly opposed to the discharge of fecal matters into streams. The self-cleansing powers of rivers, upon which so much reliance has been placed, have, he believes, been much exaggerated. Experiments at the Berlin sewage-farm have proved how rapidly a certain degree of improvement in polluted water may be attained, but they have also demonstrated how long and how far the evil effects of river-pollution may continue. He observed a growing tendency on the part of towns to resist the cost involved in dealing with polluted water; but, to prove its necessity, he stated that towns situated on the sea coast, where the speedy discharge of sewage into the sea would be considered the most ready way out of the difficulty of dealing with this question, were gradually calling for some better plan of disposing of their sewage. He noticed the difficulty of persuading agriculturists of the value of sewage water, and insisted on the need of providing sewage-grounds independent of the surrounding farmers, who will only take the sewage-water when it suits their purpose, and who frequently reject it when the town is most desirous of sending it to them.

With respect to his final proposition, he showed that often enough no difficulty is made when farmers apply excrementitious matters to their land, and which, when washed into streams and rivers by heavy rainfall, may give rise to wholesale pollution, whilst the least discharge of sewage into streams by towns is made the occasion of widespread complaint.

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THE ELECTROMOTIVE FORCE OF ACCUMULATORS.—M. Reynier, the well-known electrician, has made experiments on three systems of secondary battery: (1) the Planté accumulator of reduced lead, peroxide of lead, and sulphuric acidulated water; (2) the copper accumulator of lead, copper, lead peroxide, acidulated solution of sulphate of copper; (3) the amalgamated zinc accumulator of zinced lead, lead peroxide, acidulated solution. His object was to test the electromotive forces of the combinations and find there variations of sulphate of zinc. The accumulators were not completely formed. The electromotive forces were measured during charge and discharge by the method of equal deflection. His results confirm those formerly obtained by M. Gaston Planté, and are as follows: (1) In the three systems of accumulators studied, the secondary electromotive force is notably more elevated during charge than during discharge. The ratio of the smallest of these values to the greatest may be called the *coefficient of fall*. It is a factor of loss which effects the efficiency of accumulators. (2) The fugitive super-elevation of the electromotive force augments with the intensity of the charging current and the electromotive of the source. (3) In the Planté accumulator the electromotive force is at least 1.95 volts during the charging and at most 1.85 volts during the discharge. The coefficient of fall is therefore 0.95 under the most favorable conditions. (4) In the copper accumulator the electromotive force is at least 1.43 volts during charging and at most 1.25 volts during discharge. The coefficient of fall is therefore 0.87 under the most favorable conditions. The copper accumulator is that which loses most. (5) In the amalgamated zinc accumulator the electromotive force is at least 2.4 volts during charging and at most 2.36 volts during discharge. The coefficient of fall is 0.983 in the most favorable conditions. The amalgamated zinc accumulator is that which loses least. (6) In practice the losses due to variations of electromotive force will be greater than are indicated above, because the times of charging and discharging are generally more rapid than correspond to these experiments.

# THE SYSTEMATIC DIVISION OF LAND—A PRELIMINARY STUDY.

By HOSEA PAUL.

From the Journal of the Association of Engineering Societies.

When new territory, especially if of considerable extent, is put into the market, a systematic division is not only possible and practicable, but its advantages are so manifest that in modern times something of the sort is universally attempted with varying degrees of success. It is not difficult to hit upon some system that will be better than the haphazard selection that would proceed in the absence of any plan. It is so easy to accomplish this much that the subject is rarely a matter of thoughtful consideration or intelligent action, what is done being either in haste or left to incompetent hands, which circumstance may perhaps serve as an explanation why there are so few examples in laying out land, either in country or city, where the full capabilities of systematic division are fully brought out.

Systematic division might be defined to be the laying out of land into simple, regular geometrical figures, preferably rectangles of nearly uniform extent, and described by numbers recurring in regular order. It deals not only with the present, but to secure the best results it must in a measure forecast the future and provide for its needs; it must regulate yet not cramp the free play of individual action, by giving grooves and channels to lessen friction.

Among the advantages to be derived from systematic division might be enumerated:

1. By its providing for present requirements parcels of suitable size ready and convenient for examination and occupancy, without the delays caused by special selection, the consequent bargaining, and the necessity of special surveys.

2. By its recognizing the possibility of and providing for its future division into still smaller parcels.

3. Inasmuch as any regular figure will include land of varying worth, there will be a better opportunity of disposing of those portions of little value separately in connection with other parts which are more desirable. For instance, a rocky hillside, unsalable separately, may prove a useful addition to a lowland farm for pasturage, the growth of timber, or because of springs of water. In this way the seller can avoid the accumulation of undesirable remnants.

4. A regular division will better accommodate the uses to which land may be put either for the growing of crops, or erecting buildings upon. It aims to provide for "the greatest good of the greatest number."

5. Few advantages of regular division are so obvious, so striking and important as the increased ease and greater possibilities in the way of graphic representation.

6. This graphic representation is possible at the beginning, often as fully as needed for many purposes without other surveys than those actually made in laying out the divisions.

7. The surveys may be mainly shown by maps covering a considerable number of the divisions and the necessary facts of direction and distance noted in the lines themselves. Such maps can be easily copied or published, and convey almost at a glance an amount of information that would otherwise occupy a large and usually widely scattered spaces in written records of deeds.

8. Simplicity of description. The number of the parcel being given, the length of lines, acres, etc., can be omitted as information shown by the map, and usually expressed in better terms than by words attempting to transcribe from it.

9. By means of the greater ease of graphic representation the burden of taxation can be more equitably adjusted; each owner's interest can be marked off, and in this way none need be overlooked or forgotten.

10. Greater simplicity, uniformity and

certainty in the work of the surveyor, the regular divisions being used as a means of correction, adjustment and verification.

Although something of a regular subdivision of land has been possible in the United States ever since its first occupation by Europeans, very little in this direction was attempted in early colonial history, probably because its advantages were not appreciated as in later times. The early settlers in New England in this respect did not advance much beyond English customs. Much of this country is rocky and broken, and that was first reclaimed from the wilderness that seem best adapted for the plow, and the fields were consequently often of irregular outline, and the fashion of the times was to build stone walls after the English practice, not only for their permanence, but as a good way of ridding the fields of unwelcome bowlders. An inspection of village plans will show great irregularities, and to one accustomed to regular lots and rectangular crossing of streets such plans are a curiosity. Not only are the lot lines twisted about to an unnecessary extent, but the sides of the streets or highways are not always parallel to each other, or for that matter to anything else, but each side winds about regardless of its fellow.

In the State of New York there were formerly large holdings of a semi-feudal nature in several counties in the central part of the State. The patroons, as the holders of these large estates were called, leased out their land, refusing to sell. In the course of time the tenantry became dissatisfied and the landlords not yielding to their demands, the famous anti-rent agitation of 1840-46 was inaugurated, and though there were some agrarian outrages, the movement, which in some countries might have amounted to an outbreak or insurrection against constituted authority, took a political form and became a complete success, effectually breaking up such an unrepublican system. It is believed that the division of this territory is quite irregular.

One of the earliest examples of division by number on a large scale is that afforded by the Holland Land Company's large tract, which embraced several million acres of land in Western New York and North-western Pennsylvania. The name is derived from the fact that among those who, at various times during the progress of the revolutionary war, loaned money to the Continental Congress, were certain bankers in the old and wealthy Dutch city of Amsterdam. After the close of the war, being without other resources of repayment, this large territory was granted to a company composed of Harm Jom Huidekoper, Wilhelm, Willink and others. The divisions were termed tracts and designated by number. They usually contain about four or five hundred acres, being about a mile in length, a common form being a mile east and west by fifty chains north and south. The dimensions and directions seem, however, to be often varied without other apparent causes than those arising from imperfect surveys. The shapes of the tracts were sometimes made long and narrow to secure a frontage on some river, as is often done in the early French grants so common in Canada, and sometimes recognized and perpetuated in our public land surveys.

A very common practice in the wild and mountainous parts of Pennsylvania is to divide the land into tracts containing about a thousand acres. In some places, however, the State lands do not appear to have had any division at all. To encourge sale and settlement a sort of pre-emption or homestead right was given to whoever would settle and improve them. In due time an official surveyor would be called up to mark out a tract of such shape as might strike the settler's fancy, the main object being to inclose as much good land as possible, and leave out what seemed to be sterile and worthless.

Very often these claims were voluntarily abandoned before being paid for, a not uncommon thing in pioneer experience, and subsequent settlers ran new lines more in accordance with their own notions, but when once established they had to be followed by others. As may be imagined there was and still remains some confusion and overlapping of lines, and many nooks and corners were left out until the land became more valuable. Though these surveys were recorded, complete maps of such territory are very unusual, and some of the land is untaxed. The unit of measurement used is

the perch, divided into tenths, and for every acre sold six per cent. is added or "thrown in," for instance, an hundred and six acres is described as one hundred acres, "together with the usual allowance of six per cent. for roads," etc. Without this allowance it is called strict measure.

The Western Reserve of Ohio is a district which is comprised of the counties of Ashtabula, Trumbull, Mahoning, Lake, Geauga, Portage, Cuyahoga, Summit, Lorain, Medina, Erie and Huron, with a part of Ashland. About the beginning of this century this territory passed into the hands of the Connecticut Land Company, which was organized in 1795, with a capital of $1,200,000, there being 4,000 shares of $3,000 each. The purposes of the organization were merely temporary, inasmuch as instead of selling out the land, and dividing the proceeds, a partition of the same was at once instituted and carried into effect. Townships five miles square were laid out and numbered as ranges and towns, the ranges westward from the Pennsylvania line and the towns northward from the forty-first parallel of latitude, a system similar to that just previously introduced in the survey of the public lands adjoining. The township might be called the unit of the land company's division, the theory being that the townships were of equal value, inasmuch if by reason of an undue proportion of swampy or waste ground any one of them was deemed less valuable than the others, the adjustment was made, not by changing the price, but land in another township was added to it to bring it up to the average, the process being called equalizing, and those townships which were cut up into strips or parcels for such a purpose were called equalizing townships. The townships in this way having been brought to an average value, they were drawn by lot by the shareholders; and when this was done, the main object of the company was accomplished. The only surveys made by the company, therefore, were such as were required for this purpose, dividing the townships only where necessary for equalizing, and in such further division as was necessary to put the land into market, each proprietor acted as he saw fit. Generally speaking, without noting exceptions, if a township had a single proprietor it was divided into lots of from one to two hundred acres each. If there were several proprietors, the interest of each were called tracts, being designated by number, location as to north, south, east, or west, or taking a name from that of the owner, and their further divisions were known as lots. The survey of the townships by the Land Company was begun in 1797, and mainly finished in the course of four or five years. The division of the townships was begun immediately afterward, and very generally completed in the next ten years, or by 1815.

In some cases the owners of the tracts were in no haste to make sales, and consequently no division was made until a recent period. Other owners made sales from time to time without ever making a general survey or adopting a uniform plan.

In Summit county is the Portage Path, an ancient Indian trail about eight miles long connecting the waters of the Cuyahoga and the Tuscarawas. This trail passes near the city limits of Akron and crosses the water-shed between the St. Lawrence and Mississippi basins. At the beginning of this century it was a well-worn footpath through the forest, and as such it had been used by the Indians for a longer time than their traditions extended. It was so well-defined a highway that it was often a dividing line between tribes, and for a number of years was by treaty the line of white supremacy. Probably every vestige of the trail itself has now disappeared, but by surveys its location is fairly established and marked, a necessary circumstance, inasmuch as this ancient trail of the savage is the line of individual ownership today.

That part of the Reserve embracing 500,000 acres covering Huron, Erie and parts of Ashland and Lorain counties, which is known as the Fire Lands, was granted to the inhabitants of New London and other towns in Connecticut, who had sustained losses mainly by fire from the incursions of British troops during the revolutionary war. The loss of each individual having been determined, their interests were placed in the hands of trustees who, following the example of the Connecticut Land Co. in the other part of the reserve, made an actual partition of the land and distributed it by

lot, the partition, however, affording much smaller parcels. The first division was to cut the township into four quarters called sections, and numbered thus—

Each section would therefore contain about four thousand acres.

These were again subdivided into tracts and lots, and although no great uniformity was observed it had the advantage of being done by one authority and at one time, and what is of scarcely less consequence, very fair maps were made and the field notes, etc., generally preserved, and are now extant in a good state of preservation at the recorder's office of Huron County, the original limits of which formerly comprised the whole of the territory in question.

In this respect at least the trustees discharged their duties more faithfully than did the Connecticut Land Co., who were very remiss about getting up maps, having field notes kept, much less to preserve them; and as for the private survey of townships, the taking of field notes and the compilation of maps was something that rarely received proper consideration, and sometimes none of any consequence were ever prepared, others have been lost and others are comparatively inaccessible to the public, being in the hands of private persons, who consider them little better than old rubbish that they are gradually getting rid of. The only systematic attempt to collect these maps and papers is that made by the Western Reserve Historical Society.

The execution of these early surveys was often very imperfect. The instruments used consisted of a two-rod iron chain and a small compass with a needle four or five inches long. Though the surveying party usually consisted of but four men, besides cooks, packmen, etc., the lines were run with great rapidity, sometimes ten or twelve miles a day. The surveyor would plant his Jacob staff in the ground, place the compass upon it, level it with the needle (the only means afforded), then setting it in the desired direction, instead of sighting to a pole or staff held by an assistant, would pick out such tree or other object ahead that was on or near the line of sight. A glance was usually sufficient, and then the surveyor would set out to the point observed to repeat the process, while the chainmen and the marker would follow after, the latter blazing such trees as were within his reach. Though the country was thickly wooded, it was often comparatively free from undergrowth. It was often possible to see to a considerable distance, so that with a little care the lines could be made straight for some distance; and, the ground often being nearly level, could be measured with considerable accuracy. Such results were, however, rarely attained, and in many instances the imperfections are so great as to be best accounted for by the old traditions, that the whiskey carried to prevent the bad effects of the bites of the then numerous rattlesnakes sometimes got into the heads of the whole party without improving the character of the work.

It is doubtful, taking into consideration the better general surface of the country, whether the actual work of the surveyor on the Western Reserve of Ohio was as well done as on the Holland Tract, and it is believed the records are not as perfect; but there was one great advance made, and that was in laying out regular numbered townships. Both were faulty from the absence of any well-defined plan of numbering tracts, and still more so in the neglect of the consideration of future subdivision.

One feature of the partition of the Rerve seems to be especially novel, and that was the ingenious method of equalization heretofore described, by which the units of division that had been determined upon were kept uniform. This principle could be adopted with advantage in a great variety of circumstances where land is partitioned off in severalty among those who have hitherto owned it in common. In attempting to adjust the different interests to the varying value of the land, regularity and symmetry of form are usually lost sight of, and thus a permanent disfigurement of outline may result from a merely temporary cause, that might be easily avoided if, instead, a regular division is made, with uniform units, making the divisions of average value by add-

ing fractional parts from divisions reserved for such a purpose.

Connecticut, though perhaps the most persistent and unfair of them, was not the only claimant of the northwest territory. Virginia asserted her right to a portion of it, and there was, in consequence, granted to that State certain lands in Ohio lying between the Scioto and Miami rivers. It was set apart for the benefit of revolutionary soldiers, hence the name of Virginia Military District. No systematic survey of the territory was made, but the early settlers picked out the land wherever they could find any that was vacant, and of such shape as suited them best, those coming later being more and more restricted as the process of selection proceeded. Everywhere there is great variation in the area of adjoining tracts, and in hilly districts and along the rivers, especially, very irregular boundaries. The tracts are numbered, not, however, in their geographical order, but merely as to their time of location, perhaps the only way possible under the circumstances, but rendering the numbering of little practical use.

### THE PUBLIC LANDS.

The plan pursued in laying out the public lands need not be described here further than it provides for townships six miles square, and sections one mile square. Considering the lack of precedents the plan is deserving of the very highest praise.

There is one drawback which many must have felt if they have never complained of it, and that is the awkward way of numbering the sections. A more confusing arrangement could not have been easily found, and many persons after long practice are unable to mentally locate them in their relative order. While the ancient Hebrews wrote from right to left, and the Chinese arrange their characters in vertical columns, such is not the most convenient way for people accustomed to other methods, and it would have been vastly simpler and more easily remembered if, in numbering sections, they had been arranged as ordinary writing is—something as shown in the plan in the margin.

In this plan, which might be termed a natural or obvious one, the numbers increase downward by regular addition of six. This matter of numbering is here alluded to, not because a change is now recommended, or because the plan shown is a perfect one, but simply to call attention to the general subject of numbering—something not without its importance—as an instance where correct principles are entirely disregarded. The system is, however, uniform throughout a vast territory, and once learned, in connection with the more simple notation of the meridians, parallels, townships and ranges, locations are easily described, and as the sub-division of a section is well provided for, definite descriptions of small parcels of say ten acres can be given without reciting the length or direction of a single

| | | | | | |
|---|---|---|---|---|---|
| 1 | 2 | 3 | 4 | 5 | 6 |
| 7 | 8 | 9 | 10 | 11 | 12 |
| 13 | 14 | 15 | 16 | 17 | 18 |
| 19 | 20 | 21 | 22 | 23 | 24 |
| 25 | 26 | 27 | 28 | 29 | 30 |
| 31 | 32 | 33 | 34 | 35 | 36 |

line. This has greatly simplified the work of the conveyancer, the abstracter, the assessor and tax gatherer, as well as that of the surveyor. For the ordinary operations of the surveyor the sectional system is admirable. The original field notes and maps are a matter of public record, and show very full details of the length of lines, the meandering of streams and a description of the corners and bearing trees. The corners of a section or quarter section once established the subdivisions are matters of bisection and distribution, and thus working from the whole to parts, bearing a numerical ratio to it, great accuracy is possible, in particular those due to imperfect standards of measure can be almost entirely eliminated.

In an open or level country all of the corners along a section boundary can generally be put into a single line, thus fixing the position of a line by points

beyond it in its own direction, and not depending on cross measurements or rectangular co-ordinates. In wooded or hilly districts the half-mile points can be used instead, and the intermediate points put in the same way without any unreasonable expenditure of time in running long lines.

The improvement that might be made in the general plan of laying out the public lands will not now be discussed. The more noticeable faults are not so much of deficiency of plan as those caused by defective execution of the surveys. Being done on the frontiers of civilization, the work is at best done at a disadvantage, and often with considerable hardship, if not actual peril to those engaged in it. There is little inspection by competent officials, and still less criticism through the effective process of public opinion. As there is competition the work must be done cheaply, and of necessity rapidly. Patient, thorough work takes time and money—sometimes so much that an accurate survey would cost more than the land was worth.

These contract surveys are, however, often erroneous beyond all excuse, and the marking of corners little more permanent than the surveyor's own footprints; and, worse still, though full sets of field notes were returned, they were made up from imagination, no survey having been made at all. It is generally possible, however, to make resurveys, and thus obviate some of the inconvenience arising from this source.

That there should be great precision is not often possible under the circumstances under which the most of such surveys are made is no doubt true, and the absence of it is not of itself so serious a matter as many are apt to believe. Indeed, the thing of most importance besides the prevention of unnecessary and easily prevented errors, is the conspicuous and permanent marking of corners. This well attended to, minor inaccuracies of measurement and area may be overlooked as a matter of less importance, and which can be attended to and improved upon as increased values justify more accurate surveys.

That the plan of laying out the public lands is perfect would not be claimed. Doubtless something of value could be learned from the experience of Canada and other countries which have had occasion to solve the same problem. In Canada the principal of decimal division is recognized, but a description of the system is omitted from lack of time to fairly present it in these limits.

But the sectional system is so far in advance of any examples previously afforded in any of the older States, it might be termed, in comparison, perfection itself. Nor is it believed on an average that its execution is worse. It is interesting to observe after the lapse of thirty, forty or fifty years, or even longer, since the settlement of particular districts how closely the lines of ownership follow the section lines.

Among the earliest of these sectional surveys are found in Columbiana, Carroll, Stark and other counties in Ohio. Though over eighty years have passed, and several generations have occupied the soil of these important and thriving civil communities, the sectional divisions are by no means supernumerary or obsolete, the most noticeable variations being found in the hilly districts, where the roads are necessarily winding, and in some instances the deep ravines have been used instead.

In the more level districts the variations are exceptional, and in many counties in States farther west such variations are almost unknown.

The credit of designing the public land system has been usually awarded to Col. Jared Mansfield, Surveyor General of the United States. The great Jefferson, it is said, also honored the subject with his attention, and doubtless each of these and others did something toward perfecting and shaping the plan adopted, but Col. Charles Whittlesey, an authority of the very highest respect, in his valuable paper on "Ohio Surveys," recently published,* is inclined to award the palm to Thomas Hutchins, as the man in whose brain the conception first found lodgment, and who was one of the instruments in having it adopted. The honor is a great one, and his name may be fitly listed on the roll of great benefactors of mankind.

Hutchins was a captain in the Sixtieth Royal Regiment, and engineer to the expedition under Col. Henry Bouquet, in 1764, during which time, in the lonely

* Transactions Wes. Res. & N. O. Historical Society, Tract No. 59.

forests of Ohio, remote from the scenes of civilized life, and from contact with men or their accumulated experience, as recorded in books, he evolved in his mind the outline of the plan which was afterward adopted with but little change during the time he was geographer to the confederation, an officer similar to the surveyer-general, afterward filled by Mansfield. Hutchins died in Pittsburgh in 1788, and his remains lie unnoticed in the cemetery of the First Presbyterian Church of that city. History has certainly done him scanty justice, and it is worth while to have said a few words to do something toward rescuing his name from oblivion.*

The principle of rectangular division was adopted at a very early period in laying out American cities, the most noticeable exception being in Boston. New York, Philadelphia and Chicago are laid out with great regularity, and even in Boston the newer parts are laid out in the same fashion. In the western cities the public land system affords an excellent basis. In Chicago, for instance, many of the well-known thoroughfares, such as Madison, Twelfth, Twenty-second, State and Halsted streets, and Ashland and Western avenues, are upon section lines, and many of the other streets are upon regular fractional divisions of them.

The rectangular system has been criticized mainly from its lack of convenience when it is desired to take a diagonal course, and on account of its supposed offence to an artistic or an æsthetic taste. To meet this latter objection some towns, mostly suburban, have been laid out, an instance in point being Riverside, near Chicago, where the lines of the streets are made up of curves something in the manner of the walks or drives in a park or cemetery. However well such an arrangement might suit for residence with ample grounds, it might be quite another thing for small lots and business centers. It would at least require greatly increased care in making the surveys, or the restoration of lost lines be very difficult and uncertain.

Some writers urge that the web of the spider offers us a most admirable plan, and the national capital has been laid out with some reference to this theory, another instance being the little city of Watertown, New York. In Cleveland there is also something of the same sort, though fan shape would perhaps better describe the flare or gradual divergence from parallelism exhibited by Superior, Euclid, Garden and Woodland streets and avenues. If it were a fact that everyone must daily or constantly go to a certain point—say, for instance, to the post office—no arrangement could be better, and anything else would be so exasperating as to force the opening of new avenues to bring it about.

Such is rarely, however, the case. The business and manufacturing interests of a modern city are often widely scattered, and rarely tend to a common focus so definite as to require any such arrangement. There are east sides and west sides, north ends and south ends, uptown and downtown, water fronts and railroad fronts—each with its own interests and attracting its special following.

The advantages of representing upon paper the ownership of land are so varied and serve so many public ends, that in a progressive community it is regarded as something indispensable. One of the first attempts of this kind was when, nearly nine hundred years ago, in the reign of William the Conquerer, the celebrated Doomsday Book was prepared. Officers were appointed to traverse the kingdom and make out a list of the landowners (loyalty to the sovereign being, of course, a condition of ownership) and their possessions, something after the manner of the modern assessor. The book is still extant, and has always been regarded as an authority almost as conclusive as inspiration, the object for which it was prepared being to inform the monarch of the resources of his kingdom, and on whom he might call for support.

It is something over one hundred years ago since the British ordnance survey was begun. Its original object was to make a map of the kingdom for the

* Since the above was written there has appeared in a recent number of this *Journal*, Vol. 2, p. 282, an interesting and valuable article on "The Origin and Authorship of the Present System of Government Land Surveys," by H. C. Moore, of St Louis. Mr. Moore has looked up at the Marietta, O., University some of the original papers of Gen. Rufus Putnam relating to the Ohio Company's purchase, in which the plan is outlined as early as 1783. To fully settle the question is a matter requiring further research. Very possibly the result of such investigation would be that the honor is to some extent a divided one, inasmuch as the plan may have been developed, modified and changed by different persons.

use of the military authorities; but, as it progressed, its scope has been enlarged, until its aim is a complete and thorough representation of the topography of the kingdom. The fullness of its detail may be judged by the scale used, that for the whole country being $\frac{1}{2500}$, and for villages and towns $\frac{1}{500}$. It would seem possible, from such a map, to make a very close approximation of the proper route for a railroad or similar public work.

In this country the preparation and publication of maps, except of the sea coast, the great lakes, and some of the uninhabited territories and mineral districts has been mainly through private enterprise, which has rarely conducted surveys of any consequence; and, where there are no regular divisions, such maps are necessarily incomplete to an extent that something more thorough and extensive is now a well-recognized public need, and, with the constant reiteration of the demand, may be set down as very likely to be early undertaken. But, whether such a general survey is made or not, the existing divisions, especially the regular ones, should be kept up. They are the framework or skeleton to which all ownership clings, and the preservation of their corners and their restoration and marking when lost may well, and ought to, receive the attention of the State, and be no longer left to the caprice of individual interest.

Whatever may be accomplished in a general way by geodetic and trigonometric surveys, the importance and necessity of which need not be undervalued, however fully and satisfactorily they may solve various interesting and important scientific questions, as, for instance, the figure of the earth, as well as serving many practical ends in map-making and the accurate determination of levels, the representation of contours, and in so many other ways justify their existence to an extent that will make their cost seem insignificant in comparison to the results attained. It is, I think, a grave mistake too generally prevalent to suppose that the lines laid down by such surveys will ever be generally relied upon to establish the lines of individual ownership. In the nature of the case the lines of a trigonometrical survey are largely artificial ones, made for a particular occasion, and, however carefully selected and good originally, in the lapse of years may become wholly unsuitable by reason, for instance, of the erection of buildings or the growth of trees. The initial points, too, will often occur in fields, or at points where no one is specially interested in their preservation or to be informed of their whereabouts, or, if often resorted to, might cause complaint from the farmer who might find it convenient to have them removed. It must be borne in mind also that were such points used, and the angles turned from them, it would still be necessary then as now to actually run, measure and mark the property lines, for that indeed is the object for which nearly all land surveys are made. In actual practice the accurate determinations of and marking of lines of the various trigonometrical surveys has not often been carried down far enough to make them convenient for such use, much of the detail work having been done with the plane table. The restoration of lost boundaries, too, is often a question of evidence, much of which can only be found on the line itself. Such is the rule of law, the final arbiter, to which all must submit when controversy ensues.

NEW METHODS OF PRODUCING STEEL PLATES.—Dr. Henry Muirhead, President of the Physiological Society of Glasgow, has recently brought before that body some particulars of a method of manufacturing steel plates for shipbuilding and boilermaking purposes which is of much interest, although its leading feature is not a novel one. It is the invention of Mr. Joseph Whitley, of Leeds, who has erected works for prosecuting the manufacture. Briefly describing the process, Dr. Muirhead said, a hollow metal cylinder, lined with ganister or other brick, revolves at high speed, the axis being horizontal. A gutter or rhone perforated with holes passes into the interior, along its whole length. Into this gutter is poured molten mild steel, which, escaping through the holes, is carried round by the swiftly revolving case, and is formed into an inner cylinder of steel of an inch or more in thickness. This cylinder, while still hot, is drawn, cut across by means of a saw, put into a rolling mill, and rolled to the length and thickness required. Writing to Dr. Muirhead, Mr. Whitley said: "Required a plate for shipbuilding; then, given a mould 5 ft. in diameter and 5 ft. long, in it I cast a cylinder an inch thick. This when taken out and cut is fully 15 feet long and 5 ft. broad. It is then rolled down to half an inch in thickness. Such a plate is then 30 ft. long and 5 ft. broad. The present mould is 9 ft. long and 5 ft. in diameter. With it I successfully cast a mild steel shell weighing about 30 cwt."

## YACHTS AND YACHT SAILING IN 1884.

From the "Nautical Magazine."

It is said that the season which has just commenced is likely to be a good one for yacht racing, that contests promise to be more than usually numerous and prizes more than ordinarily valuable. The old British cutter rig reigns supreme, schooners and yawls being scarcely heard of. It appears that the extreme types of the last few years are only giving way to something more extreme, and that the divergence between cruising craft and vessels built for purposes of racing is greater than ever. The turn-out of the present year is chiefly in the smallest and largest vessels; new three-tonners are pretty numerous, and there are three racing craft of the largest size which are likely to make 1884 a notable year in the competitions between large cutters. All of them are composite boats, and we may say that this system of construction, combining as it does great transverse strength with lightness of build, together with the advantage of coppered bottoms, is likely to keep its ground for pleasure craft, the expense of building, which is a bar to its use for merchant sailing ships, being of comparatively little consequence in the case of yachts, while its peculiar advantages in them are of greater value.

We may first mention the *Genesta*, built for Sir Richard Sutton, by Messrs. Henderson, of Partick-on-the-Clyde. Her length on the water line is 81 ft., registered length 85.6 ft., beam 14.6 ft., depth of hold 11.75 ft., greatest draft 13 ft., and register tonnage 74. She is described as having a very clean afterbody, but rather fuller forward than other recent yachts. Her racing tonnage is 80, and she carries 64 tons of lead ballast, of which 60 tons is in the lead keel. It was said that she would sail her first race in the Royal Thames match from the Nore to Dover on the 7th June, but she is at present in Cork, repairing damage sustained in her first sea cruise.

The *Marguerite*, of 60 racing tons, launched on the 12th ult., from Messrs. Inman's yard at Southampton, for Mr. Foster Connor, has a length of 74 ft., beam 13.5 ft., and draft 12.5 ft. She has a lead keel of 51 tons, and 6 tons of ballast inside. Her frames are steel, topside plank teak, and bottom pitch pine, and it is said she has a greater sail area than ever before carried over 13½ ft. beam.

The *Irex* is building by Mr. Fay, of Southampton, for Mr. John Jameson, but it would appear that she is hardly likely to be ready for the earlier races of the season. Her dimensions are—length on water-line 83 ft., registered length 88 ft., beam 15.1 ft., draught 14 ft., register tonnage 75. She will carry the heaviest lead keel ever cast, nearly 72 tons. She also has steel frames.

"These three vessels," says a recent writer in the *Times*, to whom we are indebted for some of the preceding figures, "will, with the *Eryana*, *Marjorie*, *Vandward*, and *Samœna*, make up a goodly fleet indeed of heavy weights. The *forties*, which showed such grand sport last season, are not likely to be in force, but new boats in the twenty, ten, five, three ton, and the length classes will invest the battles of those sporting sections with new life and interest. It is only reasonable to assume that a forward movement in regard to speed will be apparent in the three new vessels before referred to." Of the four vessels named as conspicuous in last year's racing, the first two will have more sail area than previously, and hence it will appear that both as regards new craft and improvements in old ones there is likely during the next few months to be keen competition for prizes.

One feature common to the three vessels, of which we have given the dimensions above, cannot fail to have escaped notice, the extraordinary amount of lead ballast and the great depth compared to beam now characteristic of the racing yacht. In smaller craft these features are even more strongly marked. Thus a new yacht at Plymouth is described by a correspondent to the *Field*, as follows:—"I judge her to be about 11½ tons displacement, strongly built, with the scantlings of a 10-tonner. She has a lump of lead of about 7½ tons on the keel. She is

over 30 feet on the water-line, with large overhang and flare abaft, and will draw 8 ft. How much farther is this 3-ton folly going?" Mr. Dixon Kemp, who has written so much and so ably upon the whole question, attempts in a recent number of the *Field* to answer the question thus put by reference to a design to which has been given the name *Ne plus ultra*. It is intended to show the longest and narrowest yacht possible under the present Yacht Racing Association rule which a crew could live in, unless, as her designer says, "a man succeeds in getting a thin crew accustomed to sleep on their sides, and who would not mind coming on deck to turn round." This *reductio ad absurdum* of modern yacht construction, described by Mr. Kemp as "the plank on edge," has a length on water line of 38.5 ft., beam 3 ft., and draft 11 ft., her load displacement being 19.8 tons, of which 15 tons is lead ballast on the keel. The tonnage by the Yacht Racing Association rule is, as we have indicated, 3 tons; in fact, a start is made with 3 ft. beam, and then to give 3 tons by the formula,

$$\frac{(\text{length} + \text{beam})^2 \times \text{beam}}{1{,}730}$$

the length necessary is 38.5 ft. By the Royal Thames rule, which is even still more applicable to present types, and of which the formula is

$$\frac{(\text{length} - \text{beam}) \times \text{beam} \times \frac{1}{2}\ \text{beam}}{94}$$

the tonnage is 1.7, while by the sail area rule, at present admitted as an alternative rule by the Association, the tonnage is 12. The formula for the sail area rule is

$$\frac{\text{length} \times \text{sail area}}{6{,}000}$$

In each of these rules the *length* is length on the water-line. From what we have said it will be thus evident that yachtsmen are as far off as ever from a system of measurement which will give an equitable basis for time allowance, and that the present methods are but a very small improvement upon the old one. The alteration of the time-honored rule of the Royal Thames Club, which was for long the standard everywhere, was rendered necessary by the appearance of the *Jullanar* a few years ago. In the old rule the length was taken from stem to stern-post, and the *Jullanar*, of which a detailed and interesting description is given in Mr. Kemp's "Yacht and Boat Sailing," has about 10 ft. of immersed counter abaft the post. To meet this new departure it was decided to measure length on the water-line. At the time that further changes in the yacht tonnage rules were under discussion, we suggested, not as a theoretically exact rule, but as a good practical solution of the difficulty, a system of measurement by which the product of the three important dimensions—length on the water-line, beam, and draught—should be multiplied by a factor, determined so as to give an average result not far from the present nominal tonnage of vessels of reasonable form and dimensions. This, we still think, would have been better than the rule which was adopted, and would have been at least more easily ascertained than the sail area tonnage, now so extensively advocated, although there can be no doubt of the fact that the sail area rule is a much better measure of size than anything else at present in force.

The ideal system of yacht measurement was recently described by Mr. R. H. Froude, an eminent authority on scientific questions connected with naval architecture, as one which should "handicap all tendencies to depart from an ideal form and fitting up of a yacht." These few words indicate the whole difficulty, as probably no two yachtsmen would agree as to what was an ideal yacht. It is in the nature of things, that whatever rule is adopted, there will still be racing yachts and cruising yachts, and that the vessel which excels in one respect will be at a disadvantage in the other. Still there can be no doubt of the faultiness of a rule which induces a designer to use up so large a portion of his displacement in carrying an immense lead keel, thus uselessly increasing the expense of the vessel, for no commensurate advantage. While remarking upon the prominent defects of the present type of racing yacht, it must, however, not be forgotten that they have usually a very large range of stability, and are, in fact, life-boats which it is impossible to capsize.

Mr. Froude suggests as a remedy for the existing state of things:—"(1.) That a committee of thorough yachtsmen should

decide, among existing forms of yachts, which are eminently their ideas of cruising yachts. (2.) That by obtaining practical tests on different days in various states of weather, purely empirical diagrams should be drawn up, showing to what extent additional length should be handicapped, to what extent iron construction against wood should be handicapped, to what extent difference of area of sail should be handicapped." He also suggests that "with regard to area of full sail, it seems to me that to obtain the sail-carrying power by heaving a yacht down to her sailing inclination, with a tackle attached to a spring balance, is a much more satisfactory method of working to the same object, as it leaves the yacht free to adapt her canvass to the weather and points of sailing during the race." What Mr. Froude thus proposes to measure is the inclining moment of sail area, corresponding to a standard angle of inclination. This depends upon the area and the height of the center of effort of the sail, and thus includes the element of height of sail twice, but a rule could be framed whose practical effect would be to eliminate the extra dimensions.

It is said that during the present season a number of clubs will try the accepted sail area rule of the formula we have given; the principal objection to it on the part of yachtsmen is that the vessel's tonnage is subject to alteration by change in her rig. The grounds upon which we advocated a rule based upon the three principal dimensions of the vessel were, that sail-power depends upon length and stability, while stability depends npon beam and lowness of the center of gravity, the latter in the case of ballasted vessels depending upon draught. A good racing tonnage rule is certainly a *desideratum*, and we should think the yachting community will not much longer be without one. No rule can, we think, permanently place comfortable pleasure craft on an equality with racers, but it would be easy to reduce the present wide disparity, and afford room for the development of other and more desirable things than heavy lead keels. There is the further objection to racing yachts of the present type, that the lead keel and its connections necessary, make the vessel very costly, while as things have gone of late years she is only successful till a designer produces a vessel with still more lead, and then the ex-racer is next to useless.

Mr. Dixon Kemp's "Manual of Yacht and Boat Sailing" takes rank as the standard work upon the subject, and must, since its first publication, have been of invaluable assistance to owners of pleasure craft, from the dimensions of the racing cutters we have been describing to the tiny sailing canoe. In the earlier editions there were sections treating of yacht-designing; they, however, have since been embodied in a separate work, and, in its later form, the Manual has been entirely devoted to those parts of the subject which are of interest to the practical yachtsman. No pains have been spared to make the work a complete guide to the selection, management, and racing of pleasure craft of all sizes and descriptions. There are, moreover, special chapters upon local types of boats and yachts, showing how special forms of vessels have arisen either from local exigencies or fashions.

Mr. Kemp begins by advice as to the selection of a yacht, and comes to the conclusion that for cruising craft the rig should be cutter up to 80 tons, yawl from 80 to 150, and schooner above that tonnage; for racing craft he favors the cutter, but recommends a moderate tonnage, and, in view of our previous remarks upon the subject, we should certainly endorse this recommendation. Valuable information is given as to examination of the vessel, and as to the value of classes in Lloyd's Yacht Register, and in regard to the equipment, there are tables of sizes and illustrations of the various items. One chapter is devoted to the special question of seamanship, and another to the special management required in yacht-racing. By the title of Mr. Kemp's book, *boat* as well as yacht-sailing is implied; and accordingly much information is given upon canoeing, this part of the work being entrusted to specialists. Mr. Baden Powell contributes the section on canoes of the *Nautilus* type—that is, craft fit to sail on comparatively rough water, having good sheer, and yet coming under the definition of a canoe which he tells us "is a vessel propelled with a paddle or with sail by a person or persons facing forward: she is a vessel capable of navigating shallow water as well as open rough water, and she is a vessel not too large or heavy for land portage by two men when her

ballast and stores have been removed." Details of the construction and designs are given of several canoes of the *Nautilus* type and also of the *Pearl* type, the latter being written by Mr. Fredwen the designer of the original *Pearl* sailing canoe. Designs are also given of Mersey sailing canoes, and of an American river canoe.

Among the local types of yachts illustrated, are those on Lake Windermere, remarkable for the long overhang counter and heavy lead keels, the yacht of the Norfolk Broads of an almost opposite type, yet both carrying a large spread of sail. The remarks upon the Itchen boats are of additional interest from the fact that the standard of value for competitive sailing among them has been length only. Other notable local craft of which descriptions and designs are given are the Clyde yachts, Penzance luggers, and a large number of center board boats of various types and sizes. There is a chapter on ice yachting, in which the singular phenomenon of a vessel sailing faster than the wind is elucidated, and an appendix contains a dictionary of useful information on yachting. We believe that another edition of this valuable work is announced for the present season.

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## ON THE GASES SEPARATED IN STEEL CASTINGS.

By F. C. G. MULLER.

Translated for Abstracts of Institution of Civil Engineers.

The author in this paper replies to the criticisms on his former work contained in Mr. Pourcel's paper, read at the Vienna meeting of the Iron and Steel Institute, and to the remarks of Mr. Windsor Richards upon it. After pointing out that the cavities in unsound ingots are not the result of the enclosure of gas-bubbles in a liquid, but tubes resembling those of worms produced by the separation of gas from previously solidified metal, having their longer dimensions perpendicular to the cooling surface, he states that two theories of their origin have been proposed. The first of these, called the absorption theory, supposes that the fluid metal takes up gases from the air or the fire, which are given out again on solidification and cooling. Such an action is known to take place with silver and copper, and an analogous action is seen in the freezing of water, where the dissolved air separates and forms cavities very similar in appearance and radial arrangement to those of an unsound steel ingot. It is not necessary to suppose that the whole of the gas so taken up is separated, a portion being retained at a temperature far below that of solidification. In this respect the first separation of gas represents the first crystallization of mixed salts from a solution, and the residuum, the salts retained by the mother liquor.

The second theory, characterized by the author as the reaction theory, supposes the separated gases not to be previously contained but to be formed at the moment of separation by the reaction of dissolved oxide of iron upon carbon. This requires the gas in the pores to be of a definite composition, namely, carbonic oxide, while in the absorption theory no particular composition is specified. The part may be played by carbonic oxide, as well as by any other simple or compound gas.

The second theory is considered by the author as opposed to many observed facts. For instance, completely decarburized and overblown metal, when cast without any additions, gives off more gases in setting than any other kind, their volume being many times that of the metal, which cannot be due to reaction, as there is no carbon present.

Mr. Pourcel's explanation of the effect of silicon in producing sound castings, by its reducing action on dissolved oxide of iron, producing silica instead of carbonic oxide, is contrasted with the circumstance that German Bessemer steel is rich in silicon, and yet unsound ingots are common. Part of the author's experiments were made with steel containing 0.3 to 0.6 per cent. silicon and 0.8 per cent. manganese, and in the direct Besse-

mer process, steel with 1 per cent. of silicon is often produced, which rises so quickly that there is often scarcely time to stopper the moulds. Another fact is that in recarburizing highly silicized steel with spiegeleisen, a violent evolution of carbonic oxide takes place, showing that the reaction of carbon upon oxide of iron is effective even when the bath contains a notable quantity of silicon. In some cases the author has found an actual increase of silicon after the addition of spiegel or ferro-manganese, showing that only the carbon and manganese of these compounds were effective as deoxidizing agents. Again, in the ordinary German Bessemer process there is no sensible oxidation of silicon so long as carbon is present, its behavior being, in fact, similar to that of phosphorous in the basic process.

Although, therefore, there is no ground for supposing that the mere presence of silicon in the metal hinders the formation of gas pores, it is otherwise when an addition of a silicon compound is made before casting. This the author considers to be a proof of the accuracy of the absorption theory, as showing the phenomenon to be merely physical in its nature. Silicon increases the solvent power of steel for gases, so that the bath, when previously saturated, becomes only partially so by the addition, and therefore separation is prevented. Of course, if the opportunity is given, it may become saturated a second time, and its solvent power may be increased by a second addition of silicon, and so on. The latter statement is only put forward as a possibility, as there is no experimental evidence to show whether the solubility for gases is proportional to the content of silicon. It is more likely that an additon of 0.1 per cent. to a bath containing none may be more effective than an increase of an original 0.5 to 0.6 per cent. The first case is that of the Martin process, where highly silicized compounds are most effective. This simple explanation, the author points out, has not only been previously published by himself, but was given by Mr. Gautier a year since, at the meeting of the Iron and Steel Institute.

That small additions of foreign elements materially influence the solubility of gases in metals is proved by the analogies of copper and nickel. These metals, which, when chemically pure, give porous ingots from the separation of carbonic oxide and hydrogen; cast perfectly sound when an addition of 0.1 per cent. of lead is made to the first, and of 0.12 per cent. magnesium to the second when melted. It seems, therefore, extremely probable that silicon may have a specific action upon iron of a similar character.

The author then reviews the various analyses of gases given off by steel, commencing with those of Troost and Hautefeuille, in 1873, Parry's in 1874,* Regnard's in 1877, and his own in 1878-79. These are repeated at length, on the ground of their having been imperfectly reproduced in foreign journals.† The general result of the whole of these is to show that the gases liberated, whether by heating the metal in vacuo or boring it, consist essentially of 85 per cent. hydrogen and 15 per cent. of nitrogen. Mr. Stead's experiments, made for Mr. Windsor Richards in 1880, give an essentially similar composition. The supposition of the latter observer, that the hydrogen may be due to the decomposition of water by the friction of the metal against the borer, is shown to be untenable, as the shavings taken off when steel was slowly bored in air showed no tendency to tarnish, and if, therefore, there was no disposition to take up free oxygen, it is not likely that they would decompose water in a specially cooled apparatus in order to become oxidised. Even such a tendency, if it existed, would be checked by the reducing atmosphere produced when the first few centimeters of hydrogen had collected in the bore-hole. The enormous volume of gas, removed in Stead's experiment with the blunted borer, amounting to about eleven times that of the metal, shows that the whole of the intermolecular gas has been removed, owing to the metal being practically pulverized. This, however, is in accordance with the fact previously pointed out by the author, that the amount of gas removed depends upon the thickness of the shaving. That the volume of these intermolecular gases should be so large is, however, a new and important discovery. Parry, by heating in vacuo, obtained only a three to fourfold volume. The author considers that

* This should be 1872.
† The account is the same as that given in the Min. Proc. Inst. C. E., vol. lx., p. 499.

these gases are actually alloyed in the steel; that nitrogen is actually so contained is proved by the admirable experiments of Allen, and he is now at work upon a method for the quantitative determination of hydrogen. Both elements are integral constituents of the steel, and possibly may exercise an important influence upon its physical proportions.

Carbonic oxide is also contained in the intermolecular gases, but as it is only given off at a red heat, and not in the boring experiments, it is evident that it forms a more stable combination with the metal than either hydrogen or nitrogen.

In conclusion, the author points out that the results of some fifty experiments, made by different observers in different ways, show that hydrogen is the principal gaseous constituent obtainable from steel, whether in the gases from the molten metal, the contents of the cavities in the ingots, or the intermolecular gases of the compact metal. Nitrogen is a never-failing associate, while carbonic oxide is invariably subordinate in importance to hydrogen, and is never found in the larger cavites. It is most probable these gases are due to the decomposition of steam in the air or gases of the furnace, and that their effect can be neutralized either by the addition of silicon, so as to increase the solvent capacity of the metal, or that they can be separated mechanically by the rapid evolution of another gas on the addition of spiegeleisen.

## REPORTS OF ENGINEERING SOCIETIES.

ENGINEERS' CLUB OF PHILADELPHIA.—A special business meeting was held in Philadelphia, June 21, President Wm. Ludlow in the chair.

Prof. L. M. Haupt, Chairman of Committee on Resolutions with regard to the proposed change in the United St tes Coast and Geodetic Survey, reported the following, which were unanimously adopted and the Committee discharged:

"*Whereas*, We, the Engineers' Club of Philadelphia, have learned that a bill has been drafted in the sub-committee on Sundry Civil Appropriations of the United States House of Representatives, the purport of which is to merge the United States Coast and Geodetic Survey into the Navy and Interior Departments;

"*And Whereas*, Under existing laws and regulations, such a practical dissolution of this distinguished and efficient organization must result in a lowering of the high standard attained, without producing any equivalent on the score of economy or expediency;

"*And Whereas*, The changes now proposed were tested, both in 1834 and 1851, and in each instance proved a failure; therefore it is

"*Resolved*, That in the opinion of the members of this organization the proposed change in the status of the United States Coast and Geodetic Survey would be prejudicial to the best interests of the government service.

"*Resolved*, That the members of Congress representing the state of Pennsylvania be, and are hereby, respectfully and earnestly requested to oppose the passage of this bill."

The Secretary having arranged therefor, copies of the above were forwarded immediately after the meeting.

The Secretary reported upon the relations of the Club to the Custom House and Post Office, but delays his final report, as certain matters in relation thereto are still subject to controversy. Documentary and other evidence is on file, however, to show that the following are among the remarkable conclusions which have been reached by our government officials.

By the Custom House:

"1. That engineering societies, the objects of which are 'the professional improvement of its members, the encouragement of social intercourse among men of practical science, and the advancement of engineering in its several branches,' are *not* scientific societies.

"2. That the liability of a package to duty depends upon its size.

"3. That the *Transactions* of the Institution of Civil Engineers of England *are* subject to customs duty, and that the *Transactions* of the North of England Institute of Mining & Mechanical Engineers *are not*.

"4. That the *Transactions* of the Institution of Civil Engineers of England are worth about three cents each. *Sic Transit 'ad valorum' mundi!*)"

By the Post Office:

"1. That while *two* cents per pound is the rate accorded to the general press, including certain pictorials about which all respectable people seem to have one opinion, a society the object of which is the dissemination of useful knowledge, must pay one cent for each two ounces and fraction thereof, or over *eight* cents per pound, on a publication issued as its own proceedings.

"2. That this Society is not entitled to 'the same low rates that are extended to the general public' under the recent law, making one cent for four ounces the rate at which an individual can post a newspaper."

The report of the Secretary was referred to the Board with power to act. On motion of Mr. C. A. Ashburner, the club rooms were unanimously tendered to the local committee of the American Institute of Mining Engineers for their headquarters.

Mr. Horace See presented copies of indicator cards from the engines of the yacht "Anthracite," taken with 370 pounds boiler pressure per square inch, and steamship "Aberdeen," with 113 pounds, also a diagram showing curve of initial pressure of steam in the high-pressure cylinder of each vessel. The cards and accompanying table showed what the division of the work between two or more cylinders, so as to

limit the range of expansion and temperature in each cylinder during one stroke, has to do with the economy of the steam engine, and that in utilizing the high pressures we must employ an engine with a degree of efficiency proportionate to the increase of pressure.

Mr. Charles A. Ashburner presented a brief description of the anthracite coal fields, accompanied by a geological map of the region, which will be engraved and published in the *Proceedings*. Among many facts relating to the geology and mining of coal, it was stated that, according to recent measurements and estimates made by the Geological Survey, less than 30 per cent. of the marketable coal contained in the high dipping beds of the Schuylkill and Lehigh basins had, during the past history of anthracite mining, been sent to market, while over 40 per cent. had been left in the mines for roof supports, and over 30 per cent. had been thrown on the culm banks as being too fine and dirty to ship. It is encouraging to know that within the last few years the 30 per cent. of actual fuel coal obtained has, in many cases, been increased to between 45 and 50 per cent.

Mr. McCreath's recent analyses show that the best coals from the Lehigh basins which can be had in the market, do not contain over 86.5 per cent. of fixed carbon, although these coals have generally been rated about 90 carbon fuels. It would appear that past analyses have been of picked specimens, while those collected by Mr. Ashburner have been samples taken from between 100 and 200 tons of the coal loaded on railroad cars ready for shipment to market. The spot value at the mines of Pennsylvania anthracite is equivalent to 92 per cent. of the spot value at the mines of all the bituminous coal mined in the United States, and 89 per cent. of the value of all the gold and silver mined in the United States.

Mr. Ashburner also presented to the Club a very fine shaded lithograph of the topography of McKean County, Pa.

Prof. L. M. Haupt added to his recent papers on rapid transit, statistics of the growth of cities as exemplified in Philadelphia.

The Secretary presented Mr. John Marston's reply to Mr. J. H. Murphy's discussion of the formula for turnouts and crossings, presented by him to the Reference Book.

The Secretary also presented the following Reference Book contributions: "A Co-ordinate Method of Ascertaining Distances in Cities, Applied to Philadelphia," by Mr. John H. Dye; "Relative Elevations of Certain Leveling Data," by the Secretary.

The Club adjourned for the summer, to meet at the call of the President.

## ENGINEERING NOTES.

A New Paving Material.—The paving consists of bricks, 8 inches by 4 by 4, laid in hot tar on a bed of concrete, 6 inches in depth. In order to arrive at reliable conclusions as to the behavior of this paving material, it was laid down at the junction of the Leipzigerstrasse and Charlottenstrasse in Berlin, a spot over which the heaviest traffic passed; this was estimated at one thousand vehicles per hour, besides one thousand three hundred tramcars daily. The material appears to answer its purpose admirably. Granite, compressed asphalt, and wood pavement had been previously tried on the same spot, and had all required repair after having been down three months.

The numerous attempts hitherto made to render bricks tough and endue them with a high fracture of resistance against pressure, by steeping or boiling them in tar, have all failed, because the bitumen is unable to penetrate to a sufficient depth. In this case, however, the bricks are placed in a vacuous chamber, hot asphalt being afterwards introduced By this means the brick is freed from air and moisture, and absorbs from 15 to 20 per cent. of bitumen: it becomes very tough and elastic, is capable of withstanding great pressure, and absorbs no moisture.—*Deutsche Bauzeitung.*

These bricks have also been used for damp-proof courses, stable-pavements, retaining-walls, &c.

On the Removal of Deposit from Reservoirs in Algeria.—The reservoirs in Algeria are of great size, and they are of immense importance to the prosperity of the country; after being constructed under difficulties, and at considerable expense, they are in danger of being completely filled up, unless means are taken for the removal of the deposit which accumulates rapidly in them. For instance, the reservoir at Saint Denis du Sig, constructed with a capacity of 122,000,000 cubic feet, had in 1879 silted up to the extent of 24,000,000 cubic feet. The Habra reservoir, constructed in 1871 with a capacity of 105,000,000 cubic feet, contained, in 1879, 70,000,000 cu. ft. of deposit. The same thing takes place, but to a less extent, in Spain, and the system adopted for cleaning the reservoirs in that country is to wash them out about once in every four years. This, however, not only necessitates the emptying of the reservoir, but also causes the loss of the sediment, which is generally very valuable for irrigation. The method of remedying the evil applied by the Author at Saint Denis du Sig, consists of blowing air into the sediment, thereby stirring it up and allowing it to run off with the water. The air may be compressed by means of turbines driven by the water as it passes out of the reservoir, conducted through india-rubber pipes over the dam, and carried down into the mud by an iron tube which is attached to a float, by means of which it can be moved from one part of the reservoir to another. The Author describes the apparatus in detail, and states that as the result of an experiment on a small scale (using a 12-HP. portable steam-engine for compressing the air), the water, clear at first, became rapidly charged with the stirred-up sediment to a distance of 110 feet round the pipe. It is only necessary to inject the air near the dam, for, as the mud is removed from this position, a fresh supply flows down from the more distant parts of the reservoir, and this is in its turn stirred up into the water in a similar manner. The Author estimates the cost of an apparatus for cleansing a reservoir of the capacity of 455,000,000 cubic feet at £2,600.

## IRON AND STEEL NOTES.

ON THE PRODUCTION OF CEMENT FROM SLAGS.—By L. ROTH.—The author points out that Portland cement, and the slags obtained in the production of foundry pig-iron with coke, are essentially similar in qualitative composition, as is seen by the following analyses:—

| | Cement. | Slag. |
|---|---|---|
| Lime | 60.05 | 51.62 |
| Silica | 24.31 | 35.12 |
| Alumina | 7 50 | 9.53 |
| Magnesia | 1.17 | 1.58 |
| Ferric oxide | 3.34 | — |
| Ferrous oxide | — | 0.87 |
| Manganous oxide | — | 0 37 |
| Potash | 0.80 | — |
| Soda | 0.74 | — |
| Sulphur | — | 0.88 |
| Gypsum | 1.82 | — |
| | 99.73 | 99.97 |

In the slag, sulphur exists as sulphate of calcium, which would be injurious to the setting-power of the cement, and there is also a quantitative deficiency in certain bases. In order to supply these, the author mixes the slag with Bauxite and limestone, or lime, and moulds the powder into bricks, which are burnt in an ordinary cement-kiln, Hofmann's annular kiln being preferred. In firing, the heat is raised gradually from dull to bright redness, to drive off combined water and carbonic acid, and to convert a portion of the sulphide of calcium into sulphuretted hydrogen, and aluminate of lime; after which it is sintered at a strong white heat, and the resulting mass is crushed and ground in the usual way.

As the value of Bauxite depends upon its contents of free alumina, the proportion added will vary with its composition; the limits between which the finished product may be regarded as giving good cement are:

| | |
|---|---|
| Lime | 55 to 63 per cent. |
| Silica | 22 to 26 " |
| Alumina | 6 to 10 " |

The proportions used by the author, with a basic slag of the composition previously given, which falls to powder when cooled, are:—100 slag, 85 limestone or chalk, with $CaCO_3$, 98, and $SiO_2$ 2 per cent., Bauxite 15. The latter is the variety found near Giessen, which contains $Al_2O_3$, 48.5; $Fe_2O_3$, 13.52; $SiO_2$, 9.40 per cent.

This mixture when burnt gave 158.66 parts of cement, of the following composition:

| | Per cent. |
|---|---|
| Lime | 61.9 |
| Silica | 24.1 |
| Alumina | 10.6 |
| Ferric oxide | 1.3 |
| Ferrous and manganous oxides | 0.8 |
| Magnesia | 1.0 |
| Sulphur | 0.3 |
| | 100.0 |

One half of the total quantity of the sulphur in the slag was eliminated as sulphuretted hydrogen. The cement, when ground and sifted through a sieve having nine hundred holes to the inch, was of a greenish-gray color, and of strong hydraulic properties, showing no tendency to swell when once set. It is essential to the process that the materials should be perfectly mixed before firing. Slags that fall to pieces in the air should be sifted through a 900-hole sieve, but when they are not sufficiently basic they should be granulated in water when run from the furnace, and ground in the cement-mill. A small addition of soda may be made advantageously in some cases, but as a rule it is not necessary. The cement may be applied in the formation of articles in concrete with granulated slag, which possess a certain hydraulic character.—*Abstracts of Institution of Civil Engineers.*

## RAILWAY NOTES.

THE annual report of the Dominion Minister of Railways, as presented to the Canadian Dominion Parliament, shows that during the year under consideration as many as 1275 miles of railway were added to the length of road in operation in the Dominion, making a total of 8805½ miles under traffic. The railway system of Canada will within the next two years, when all the uncompleted lines are finished, comprise something over 11,400 miles. The paid-up capital has increased during the year from 415½ million dollars to 494¼ million dollars, or 19 per cent., an increase in the capital per mile completed and under construction of 17.03 per cent. The business done by all the lines in operation has grown in large proportions. The gross amount of freight carried during the year was 13,266,255 tons; and the gross receipts 21¼ million dollars. The number of passengers carried was 9,579,948, and the gross receipts showed an increase over those of the preceding year of 4¼ million dollars. The net earnings were sufficient to pay approximately a dividend of 2½ per cent. on the share and bonded liability of the roads in operation.

LARTIGUE'S ELECTRIC RAILWAY.—M. Lartigue, the well-known French engineer, has applied electricity to the traction of the panniers or cars of his single-rail tramway. This tramway is, as we stated some time since, employed in Algeria for transporting esparto grass from the interior by the traction of camels. It was an easy step from animal to electric traction, and M. Lartigue has successfully taken it. At the recent Agricultural Exhibition in the Palais de l'Industrie, of Paris, an experimental line was shown on which five iron panniers, or double cars in the form of seats, were drawn by a dynamo-electric locomotive at the rate of seven miles an hour. The total weight of the five cars and the electric locomotive was about a ton, and the maximum power required was three horse-power. The dynamo of the locomotive was a Siemens $D_6$, and the generator, which stood about 100 yards from the line, was a Siemens $D_3$ dynamo capable of developing from 5 to 6 electric horse-power. It was driven by a Herman-Lachapelle steam engine. The total length of the line was 123 meters. It was built of forty-one rails, each 3 meters long,

and comprised curves of 7½ meters radius. The locomotive dynamo was carried by a platform car or pannier, and geared with a grooved driving wheel 30 centimeters in diameter, which ran upon the rail. A rheostat to graduate the speed, switches to stop, start, and reverse the motor, and a seat for the conductor, were also carried by the locomotive car. The train was properly coupled to the locomotive, and ran on small grooved wheels. The current was brought to the dynamo by two insulated conductors, one connected to the rail, the other to the dynamo through small contact rollers in connection with the commutator. One switch was employed to start or stop the train by making or breaking the circuit; the other to reverse its motion by reversing the current. The rheostat, by interpolating resistance into the circuit, allows the strength of the current to be varied and the speed of the train to be increased or diminished as the case may be. The work was carried out by Messrs. Siemens, and under the direction of M. G. Boistel. The economy of the working is of course largely dependent on local circumstances.

An Electric Mountain Railway.—A project has been formed, writes a correspondent, and will, in all probability, shortly be executed, for uniting the Hotel des Alpes, at Territet Chillon, and the Hotel de Mont Fluery, which is situated on the steep mountain side immediately above Chillon, by an electric railway. The difference of altitude between the two hotels is 180 meters, and the system which it is proposed to adopt was put to an experimental test recently. Rails of a gauge of 50 centimeters were laid on a part of the mountain for a length of 50 meters, and with a gradient of 30 to 100. Between the rails is a rack for the reception of a toothed wheel, and at each end is a curve in order to show the advantage of the proposed system over the funicular system, which does not admit of the slightest curve. The wagon carries a dynamo-electric machine, which actuates by special gearing a toothed wheel on the axle in connection with the rack between the rails. An electric and an ordinary brake enable the conductor to modify the rapidity of descent at pleasure. The electricity was produced for the occasion, and in the first instance, by a dynamo-electric machine of 5 horsepower, actuated by a portable engine. This engine it is intended to replace eventually by a turbine, for which there is an ample supply of water. The apparatus for the transmission of the electric force to the receiver on the wagon consists of copper cables laid between the rails and alongside the rack, and the electric contact is established through the intermediary of mechanical brushes in the usual way. The carriage, in which were seated two, three, and sometimes four persons, the whole, including the carriage, representing a weight of from 300 to 500 kilogrammes, went up the steep incline at the rate of from 1 meter to 2 meters per second, and the descent was completely controlled by the ingenious combination of the two brakes. In the event of the project being carried into effect, which depends to some extent on the approval of the federal government, the journey between the two hotels may be performed in seven or eight minutes. As, moreover, water is plentiful in Switzerland, there is no reason why every fashionable mountain should not have its own particular electric railway, and the time may come when idle or infirm tourists will ride luxuriously across the Mer de Glace and be carried on wheels half way up the Jungfrau and the Matterhorn. Already the people at Geneva, encouraged by the success of the experiment at Territet Chillon (Montreux), are proposing to utilize the waters of the Arve for the ascent of the Saleve.

## ORDNANCE AND NAVAL.

The Moncrieff System.—Colonel Moncrieff read at the United Service Institution, on Friday, April 25th, to a large audience, a paper on his system of mounting ordnance. Probably many of our readers have latterly lost sight of the Moncrieff system, and have assumed that for some reason it had not fulfilled the promise of which it once appeared to be full. Some of us probably assumed that it was found inapplicable to the heavy guns now employed, or that the carriages were too complicated, or, again, that there were objections to the receivers on the hydro-pneumatic carriages. If none of these things were true it was high time that Colonel Moncrieff should call attention to this important question, which had been allowed to lapse into oblivion.

In a paper couched in very mild and moderate terms, Colonel Moncrieff explained his case on Friday. He divided the methods of mounting guns into three systems: (1) Ordinary barbette; (2) iron casemates and turrets; (3) the protected barbette or disappearing system, commonly known as the Moncrieff system. The first system—the barbette proper—exposes the men so much that it was found even in the days of the American War that barbette batteries could certainly be silenced by armor-clad vessels. Since the introduction of machine guns this operation is easier than ever. The turret or casemate system is very expensive, and the guns have, in the case of casemates, but limited scope. Consequently Colonel Moncrieff urged that his system obviously met the need of the country by providing guns which combined complete protection with all-round fire, and which offered enormous saving in money. He informed his audience that the manner in which he had been dealt with was as follows:—Successive special committees had been directed to investigate the merits of his system. He had never failed to convince a committee after a time that his system ought to be adopted; so that each committee eventually recommended its adoption. This, he said, was always the signal for the abolition of the committee. Seven committees had eventually come to the same conclusion, and had been abolished at last. Colonel Moncrieff, who was then employed by Government, was himself abolished, that is to say, his appointment was done away with, he himself being told that his services were not further required. The crowning evil was that the delay and obstruction which had taken

place have acted as a vague argument against the Moncrieff system, which it is now urged has somehow failed to succeed. The Navy had wished for the Moncrieff system, so had the Royal Engineers; the entire fault he therefore maintained lay with the Director of the Artillery Department, who refused to make the carriages, and even to buy a carriage which was to be made by Messrs. Easton and Anderson free of expense. The Colonies had asked for the Moncrieff system, which is specially suited to the case of limited numbers of men; because fewer guns are needed, owing to the fact that all-round fire is possessed by Moncrieff guns, and also that the men are very completely protected and are likely to have very few casualties. Colonel Moncrieff read a letter from General Scratchley, R.E., the adviser to the Australian Government, saying that he had always advocated the adoption of the system, and pleading that the blame of its being prevented from coming in did not lie with the Royal Engineers. It appeared that in 1872 the Moncrieff system was first approved by a committee, and for fifteen years the process of ignoring recommendations made on the score of both efficiency and economy by committees has continued. Hence Colonel Moncrieff said that he felt it needful to speak out, which he did as a man in whom all enthusiasm had been extinguished, but who had full faith that his system must eventually be adopted, although, judging from past experience, it might not be in his lifetime.

In the discussion that followed, all the speakers sided with the lecturer. Naval officers, artillery officers, and engineers appeared to be in complete accord. Mr. Anderson was called upon to speak as to the application of the hydro-pneumatic system to heavy guns, and replied that the Russians had two 40-ton guns mounted on this system, and were so highly satisfied with it that they declined to try the Albini carriage. Admiral Ryder and Admiral Sir John Hay, Admiral Boys and Admiral Selwyn, Colonel Duncan, R.A., and others spoke. It was suggested to Colonel Moncrieff that perhaps he had proved his case too clearly and unanswerably for success; he was reminded how irritating a thing this is, and he was even asked whether he could not introduce some features that would furnish his system with such faults as would make it less obnoxious and more calculated to invite sympathy.

THE "DELTA" STEAM LAUNCH.—A steam launch constructed of "delta metal" was lately exhibited at the Crystal Palace in the joint names of Mr. Alexander Dick, the manufacturer of the new alloy, and Messrs. Yarrow & Co., the builders of the launch. As it has been proved by repeated experiments that the delta metal is equal in strength, ductility, and toughness to mild steel, the plates and angle pieces of this launch were made of the same thickness as if they had been of steel, namely $\frac{3}{16}$ in. The length of the boat over all, is 36 ft., the breadth of beam 5 ft. 6 in., and the depth from gunwale to keel 3 ft., the capacity being sufficient to provide sitting accomodation for twenty-five persons. The stern, keel, and stern-post are of forged delta metal, and are scarfed together in the usual way. The angle frames are made of the same material and are placed longitudinally instead of transversely, to give greater longitudinal strength. The propeller cast in delta metal, is four-bladed, 2 ft. 4 in. in diameter, and 3 ft. pitch. The engine is of the usual direct-acting type, and of sufficient power to propel the boat at a speed of eight or nine knots per hour. The advantage of delta metal over steel and iron for shipbuilding, is that it does not rust. It is well known that a thin steel vessel, unless continually painted will rust through very rapidly. This difficulty has been found to exist to a remarkable extent in the rivers of Central Africa; in these the water, from some unexplained cause, possesses an extraordinary power of corroding and eating through steel plates. This fact is of special interest at the present moment when the rapid development of the African continent may be looked for. An important advantage possessed by delta metal for the construction of large boats, where its weight must be reckoned by tons, is that it is offered at a moderate price, and consequently the undoubted advantages of non-corrodibility are not eclipsed by a prohibitive cost.

## BOOK NOTICES

MODERN HIGH EXPLOSIVES. By M. EISSLER, Mining Engineer. 8vo. New York: J. Wiley & Son, Publishers.

In the book before us we have an attempt made at editing much useful information concerning explosives.

The compiler has accumulated, from various sources, data relative to the chemical constituents and the methods of analysis which enter into the manufacture of high explosives. He treats in detail of the preparation of nitro-glycerine, the various nitro-glycerine compounds, pyroxylin, gun cotton, nitro-cellulose, and the fulminating compounds. In the second part directions are given for using the high explosives, with special reference to the various processes of firing or igniting by means of electric machines, fuses, and similar appliances. The third division of the work is devoted to different methods of blasting, including special applications in various engineering enterprises from excavating tunnels or mining shafts, to the removal of stumps and the breaking of ice. An appendix, in which various theoretical considerations will be found, brings the work to a close. The theories of explosion, as put forth by Able and Berthelot, are alluded to and commented on.

That a book of this kind is needed there can be no doubt, and we regret extremely to find in a work, admirable in so many respects, the evidences of unfortunate ignorance or extreme carelessness. The section devoted to glycerin, which is fairly representative of the book, is so filled with mistakes and inconsistencies as to be utterly unreliable. Even a cursory glance will discover many errors in chemical nomenclature. On page 15 "three hydroxyl radicals (3OH)" should be 3(OH). A few lines further on, "the oxide of a metal like lime, lead, etc.;" lead is not an oxide, and which oxide of lead is meant? Bone black is called

bone coal (p. 25). The use of final *e* in glycerin is not accepted by the best writers in chemistry, but then no attempt appears to be made to follow correct usage in this respect. Even on the first page, Chili saltpetre is first written *sodium* nitrate and then *sodic* nitrate, thus tending at the very outset to confuse readers not familiar with the ancient and current chemical nomenclatures. We find also many instances in which the editor is inconsistent with himself; for instance, on p. 12, the table of statistics foots up 9,500 tons. while directly following it is spoken of as 7,500 tons. On the same page, glycerin is said to boil at 554° F., while on page 24 it boils at 618° F., which is correct—and why are not references given? On page 13 it is stated that glycerin crystallizes at 42° F. This is novel information, and we should be glad to know on whose authority this statement is made. There are also many infelicities and ambiguities, such as the following: "Small particles of soap *conglomerate* to small lumps" (p. 18). Sentences not easy to understand: "The method which is employed depends on the *proceeding* by which the fats are decomposed and the quantitative results of glycerin on *them*" (p. 16). In the saponification of tallow by lime, it is best to use little lime; and a high pressure, for instance, by employing one hundred parts tallow and four parts of lime. Several of the latest and most improved processes of extracting glycerin are ignored entirely.

The book, therefore, is not one that we can recommend to our readers Those seeking a knowledge of the subject will be misinformed by a perusal of the volume, while those who use it for reference will find it so unreliable that they will be compelled to throw it aside as worthless. It can only be of service to such experts as are competent to separate the good from the bad, and even to them its utility will be slight. It is, however, an excellent specimen of mechanical book making, and while it evidences the good taste of its publishers, its best function will be to serve as a sample of fine printing.

STEEL AND IRON. Comprising the Practice and Theory of the several Methods pursued in their Manufacture, and of their Treatment in the Rolling Mills, the Forge and the Foundry. By W. H. GREENWOOD, F. C. S., Assoc. M. I. C. E., M. I. M. E. With ninety-seven diagrams from original working drawings. London: Cassell & Co. 1884.

This work, one of the manuals of technology edited by Professor Ayrton ahd Mr. R. Wormell, and published by Messrs. Cassell, is a comprehensive handbook of practical information, and of the scientific principles upon which the practice is based. We may at once state that Mr. Greenwood's book appears to us to be an excellent manual of the subject, which scarcely needs comment. The book is of service to the general student of the branch of technical science of which it treats, as well as to the intelligent workman, to whom it offers a succinct statement of the scientific principles upon which depends the success of the several processes in which he is engaged. The book does not—and is not intended, we believe, to —supersede the experience and practical knowledge that can be gained only in the works; but it will be found a useful adjunct to, and contribute to a clearer understanding of, these things.—*Iron.*

THE MATERIALS OF ENGINEERING, IN THREE PARTS: PART III., NON-FERROUS METALS AND ALLOYS. By ROBERT H. THURSTON, Professor of Engineering, Stevens Institute, etc., etc. New York: John Wiley & Sons.

For this volume Professor Thurston goes very exhaustively into the history of non-ferrous metals and their alloys, giving their general characteristics and their special properties. The author's very extended and carefully conducted experiments have resulted in the composition of new brasses, kalchoids and bronzes which must be of special value to workers in alloys.

WROUGHT IRON AND STEEL IN CONSTRUCTION. New York: John Wiley & Sons.

Rules for Strength of Pencoyd Iron should be properly the title of this book. Rolling mills and bridge-building companies issue "Pocket Books" of "Useful Information for Engineers, &c.," which are not simply advertisements, but contain facts and tables very useful to engineers. Pencoyd is very slow in issuing its book, which, it must be confessed is below the average standard. The book has the merit of being clearly printed on handsome paper. Pencoyd beams, channels, angles, and other shapes are set forth at great length. One might suppose that such shapes of iron had never existed before, and that a Pencoyd circle would contain a greater area than another circle of the same diameter. There is much theoretical matter in the book which is evidently very valuable in the eyes of the author, but the experiments published do not tell us what is of practical use, that is, for instance, what is the value of an angle iron, used as a strut or a tie, riveted by one leg only to its connections. The book is too shoppy and theoretical.

THE IMAGINARY METROLOGICAL SYSTEM OF THE GREAT PYRAMID OF GIZEH. By F. A. P. BARNARD, LL. D., S. T. D. New York: John Wiley & Sons.

Dr. Barnard has rendered the public an excellent service in this essay. The advocates of the pyramid metrology are urging their claims in a way that tends to deceive those who are not sufficiently familiar with the fact that figures adroitly presented may be made very misleading. The honesty of the believers in the pyramid theories presented by Piazza Smyth is not to be questioned, but the theories are, in consequence, all the more likely to deceive, and it needed the keen analysis and the pungent satire of the present volume to check the spread of this peculiar phase of fanaticism.

The essay begins with the following paragraph: "Among the many vagaries of the human mind which history records, there is not one more extraordinary than that which has been busying itself for the last twenty years in weaving a network of religious mystery around the great Pyramid of Gizeh. If it were not a

law of fanaticism that the faith of its subjects and victims is intense in proportion as its doctrines are defiant of common sense, it might reasonably have been expected that the wild conjectures, in regard to this monument, hazarded by John Taylor in 1859, would have fallen unnoticed from the press, and would long since have ceased to be remembered among men."

These conjectures, which have been elaborated by Professor Smyth, and seriously formulated into a doctrine, affirm in general terms—that the Great Pyramid was built by Divine direction, and that its various dimensions reveal, when rightly interpreted, the size and density of our globe, the solar distance, and the length of the year; also exhibit quite accurately the periods of time separating the important crises of sacred history.

As the length of the base of the pyramid affords the starting point for numerical comparison, and as there is a remarkable lack of agreement in the measures thus far obtained, Prof. Smyth has adopted a number lying between the extreme results, but which is not a fair average of the measurements, nor does it agree with any one of them, but it fits the theory, and is supposed to be, therefore, worthy of adoption. There is much of this kind of dealing with numbers among the pyramidists. Sometimes the measurement of the pyramid is made to adapt itself to the requirements of the theory, and sometimes it is the so-called *revealed* dimension or date that suffers the strain. An example of this latter kind is afforded in a proposition of Professor Smyth's, which runs as follows:

Multiply the tenth part of the cube of the ten millionth part of the earth's polar diameter by the mean density of the earth, and the product will give the pyramid unit of capacity. (The presumed original capacity of the sarcophagus.) Dr. Barnard says of this, after presenting the figures, "The arithmetical coincidence is exact; but we shall see that this extremely satisfactory result is only owing to the happy selection of a value for the axis of the earth, which no computer has ever found, and of a number expressive of the earth's density, which no investigator of that subject has ever reached."

Coincidences of like kind are numerous in the standard work of the pyramid followers (*Our Inheritance in the Great Pyramid*), and the principal ones are analyzed in like manner.

If anything could exhibit the absurdity of the doctrine here criticised in a stronger light than the exposition of the method of establishing it, it is the satire embodied in Dr. Barnard's suggestion that a similar manipulation of figures exhibits the lunar theory in the temple of Ephesus, and the base of the hyperbolic system of logarithms in the base of the Great Pyramid.

Dr. Barnard's essay will be widely read, and most readers will learn with surprise that men eminent in science and literature have been misled by the doctrines that are therein so effectively analyzed.

DRAINAGE AND SEWERAGE OF DWELLINGS. By Wm. Paul Gerhard. 12mo., 302 pp. New York: Wm. T. Comstock. Price $2.50.

It is always fortunate when a man eminently successful in a large practice is able and willing to write his views and methods clearly, and give them to the world in a book. Mr. Gerhard has made a book on an immensely important subject, which sustains the reputation he had before earned by his masterly report on this subject to the Rhode Island Board of Health: We define a practical man to be one accustomed to successfully doing the work. Mr. Gerhard is now actively employed as chief engineer of a company making a speciality of draining houses and populous districts.

It is not easy to find a polite descriptive adjective for the man or men who first ventured to take a privy into the middle of a first-class dwelling-house. Lazy is a weak term, and effeminate is a slander on the sex. His faith was strong—faith in the efficiency of water and traps which experience and the loss of many lives has shown to be misplaced unless more care and skill than usual are exercised.

This book by Mr. Gerhard seems to go far towards undoing the mischief. It does not, like too many, spend all the strength of the writer and the patience of the reader on the evils to be avoided, but diligently and patiently labors to show how to avoid them. It is liberally illustrated with sections showing the arrangements and devices in general view and in detail. Traps old and new are dissected profusely.

We are particularly pleased with Chapter II., "Necessity of Ventilation in Rooms containing Modern Conveniences," Chapter VIII., "Drainage of Cellars," and Chapter X., "System of Internal Sewerage as it should be in a Dwelling." The work should meet with an extensive sale.

A PRACTICAL TREATISE ON ELECTRIC LIGHTING. By J. E. H. Gordon, B. A., M. S. T. E. Sampson Low, Marston, Searle and Rivington, London. 1884.

It seems to us that the author of such a book as this should treat his subject under heads, his principal heads being: (1) generation; (2) distribution; (3) utilization, and (4) tools; the sub-heads may be as numerous as the author chooses. With regard to the generation of electricity little need be said of batteries; but some attempt might be made to explain the great similarity of the various dynamo machines. They are easily classed under a few types. The continuous ring armature type includes such as the Gramme, Burgin, Brush, Schuckurt, &c. Then we have the Siemens armature type, alternate current machines, and so on. Starting with a full and complete description of the typical machine, the various modifications, the reasons for their introduction and the gains by introduction could then be successively discussed. However, it is generally admitted that, according to their own showing, most reviewers would make admirable authors. Unfortunately, when they attempt to carry into practice the views they put forward, the result is frequently a miserable failure. We shall therefore return to the real subject of this notice, showing that Mr. Gordon has, to some extent, carried out these views. After an introductory chapter on the

principles of artificial lightning, he shows the analogy between a fluid, such as water, and electricity. Although we think the way in which Mr. Gordon has introduced these analogies to be exceedingly instructive, we must not forget the caution of Clerk-Maxwell when warning his readers against the use of the term "electric fluid."

The aim of those who desire to produce artificial light economically is thus stated, p. 8, "to concentrate the heat in a solid of the smallest possible size, or with the smallest possible cooling surface." How this is done in electric light operations it is the business of the book to show. Whenever a new application of science is developed, it becomes necessary to show how the principles of the science are used in the development. Practical work needs systems for measurement, practical units of measurement, notation, and so on—a branch of the subject to which Mr. Gordon devotes some of the early chapters of his book. In these he discusses the relation of the electrical units with one another, and with the units of heat and work. The electrician finds nothing new herein, but the electrical engineer will here see the arithmetic of his work. The formulæ he has to use are collected and illustrated. Here, too, are some interesting comparisons, showing the value of the commercial electrical unit in various measures, and we are given a rule for the comparison of the prices of electricity and gas. Thus: "To compare the price of electricity with that of gas, we must multiply the price per electrical unit by 10, and the result will be the price of a quantity of electricity approximately equal in illuminating power to 1,000 cubic feet of gas."

Electricity—to use an ordinary expression—generated for electric light purposes is utilized by means of lamps. The different incandescent lamps are, in the first place, described, and their method of manufacture, the details being given and amply illustrated. Then the best known types of arc lamps are described. The requirements for the lighting of particular places are discussed, and it is shown how the lamps and other apparatus must be designed for the purpose in view. A very interesting chapter, for which the author states his indebtedness to Mr. Crompton, on carbons for arc lamps, is given, and then we come to that portion of the book dealing more directly with the generation of the current. Besides being a writer, Mr. Gordon is an inventor, and professes to follow the example of Cumberland, Maskelyne, and others, in "telling how it is done." We doubt, however, if the readers can invent, after reading this book, any more than the hearer could perform the tricks shown at St. James's Hall after hearing how they were done. Indeed, the telling is simply reduced to this: Mr. Gordon has by a process of reasoning come to the conclusion that a certain type of dynamo is the best, and he forthwith sets to work and designs such a dynamo. Now we are inclined to think that there is more than one road to travel in order to get to a new design of dynamo; and we are not at all sure that Mr. Gordon's explanation proves that his road is the best. However, there is no doubt that he puts forth the principles of magnetic induction and of the best-known machines in a clear and concise manner. All this part of the book is exceptionally worthy of close study. It abounds with hints of a very practical nature, and points out what not to do in many cases.

Before concluding this notice a word of praise should be given for the excellent illustrations and typography of the book.—*Abstract of Review in Engineer.*

## MISCELLANEOUS

THE riches of the United States from gold and silver mining may be partially estimated by the fact that the aggregate production of gold up to June 30, 1883, has been 1,632,364,670 dols. That of silver has been 598,083,217 dols., making a grand total of 2,230,447,887 dols., or £446,089,577. Reduced to the equivalent weights, the total gold output has been 78,965,572 troy ounces, or 2707.4 avoirdupois net tons, while the silver weight represents 462,590,469 troy ounces, or 15,860 tons. Putting the statistics into another form, the gold produced in the country up to the present time, if brought together, would be sufficient to load 271 ordinary freight cars; the silver, supposed to be collected as fine bullion, would require 1586 cars for its transportation. The gold would tax the carrying capacity of a large ocean steamship, while the silver would form cargoes for a considerable fleet.

THE distances, often many miles, through which gas is transmitted before reaching an engine, are such that, with any other means of distributing power, they would considerably enhance the cost of the power. But in the case of gas, it does not appear that these distances are at all a matter of consideration. Professor Osborne Reynolds thus explains this:—It takes about ten cubic feet of gas to develop 1,000,000 foot-pounds in a gas engine, whereas of air compressed in the ordinary way, it would require something like 140 cubic feet to yield the same power. Hence the comparative cost of transmission is the cost of transmitting ten cubic feet of gas against that of 140 cubic feet compressed air, and these would be about as one to twenty-five; so, as a means of distributing energy, gas is twenty-five times more efficient than compressed air.

PROFESSOR FOSTER recently read a paper, before the Physical Society, by himself and Mr. Pryson, on the difference of potential required to give sparks in air. Let V=this difference of potential, $l$=length of spark in centimeters, their experiments give approximately $V=102l+7.07$. Tables and curves of the sparking distances, potentials, and electric forces in the experiments were given. The results were got with brass balls 1.25 centimeters in diameter, a friction machine, and a Foster absolute electrometer. When $l=.142$, the electric force giving a spark was 154.76; $l=.284$, the electric force was 133.35, or less than at a shorter distance; $l=497$, the electric force was 131.66; $l=.9$, the electric force was 138.57, that is, it began to rise again.